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An ingenious ultracentrifuge cell in which equilibrium conditions can be reached rapidly on multiple samples has been described by David Yphantis of the Rockefeller Institute.

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Sedimentation of High Polymers

Of special interest to polymer chemists is a comprehensive summary on sedimentation of synthetic and natural polymers by R. L. Baldwin of the University of Wisconsin (now at Stanford) and K. E. Van Holde of the University of Illinois. The authors discuss in detail the kinds of information obtainable by ultracentrifugation, and methods used. An appendix lists polymers run on the Ultracentrifuge, solvents, and literature references.

The work appeared in the first issue of the German journal "Advances in Polymer Science"; reprints (in English) are available from Spinco.

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General Motors Research Laboratories Warren, Michigan

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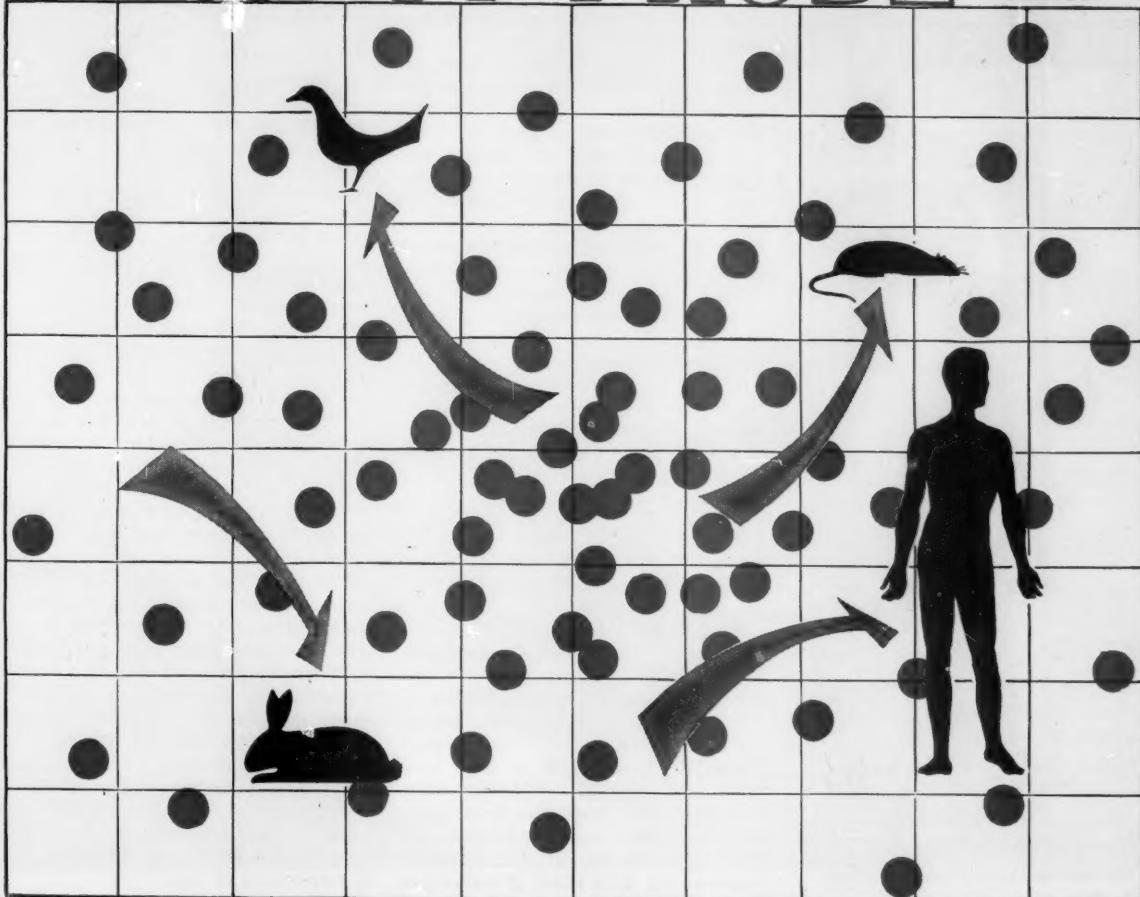
	a	b
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Cobalt	1.85	1.83
Nickel	1.84	1.83
Supermalloy	1.91	1.91

Comparison of (a) gyromagnetic ratios measured in the new Kettering Magnetic Laboratory with (b) corresponding ferromagnetic resonance measurements. These ratios would equal 2 if magnetization were due only to electron spin, or 1 if due only to orbital electron motion.

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Letters

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Investigators, institutions, or sponsors of meetings should write to me for information concerning submission of material. Manuscripts offered for publication as a National Cancer Institute monograph should conform to the instructions to authors appearing on the inside back cover of the *Journal of the National Cancer Institute*.

MICHAEL B. SHIMKIN

National Institutes of Health,
Bethesda, Maryland

Progeny Yields in *Drosophila*

In a recent report by W. C. Levengood and M. P. Shinkle [*Science* 132, 34 (1960)] regarding environmental factors influencing progeny yields in *Drosophila* it is stated that "atmospheric pressure effects on progeny yields . . . appear not to have been previously reported." Although comparatively little work has been done, there have been some publications applicable to the subject. For example, Stephen and Bird [*Can. Entomologist* 81, 132 (1949)] studied some effects of different pressure levels on oviposition in the cabbage worm, *Pieris rapae*. Moreover, in reviews by Uvarov [*Trans. Entomol. Soc. London* 79, 1 (1931)] and Wellington [*Can. J. Research* 24, 51 (1946)] reference is made to Pictet's studies on pressure effects on emergence of *Pieris* adults. Although Levengood and Shinkle seem to have been concerned principally with numbers of progeny in their experiments, the observations by Pictet and by Stephen and Bird are directly applicable to experiments concerning progeny yields. Stephen and Bird found increased oviposition in insects exposed to relatively low pressures (900

to 930 mbar) as compared with that at higher pressures. Pictet reported that pressure changes might contribute to the success or failure of *Pieris* to emerge from the pupa. Parental oviposition and subsequent emergence from the pupal stage each may influence the final number of adult progeny. Incidentally, the results of Stephen and Bird (increased oviposition at lower pressures) do not support the data of Levengood and Shinkle (decreased number of progeny from matings during lower pressure).

Levengood and Shinkle also report results of rearings of *Drosophila* in an electrical "field." They found a lack of correlation between numbers of progeny and pressure level during mating, under the influence of the field. However, they do not give the amount of variability in the progeny data—a statistic which would aid in interpretation of these data, particularly since so much stress is placed upon this negative effect. The field presumably was developed through and around the culture medium. It would also have been helpful, therefore, if some indication of the dielectric capacity of the culture medium were given, since the dielectric capacity is inversely related to the field strength within the medium.

The authors appear not to have been too sure of the difference between an electrical field and an amount of electricity. For example, they state that the "electrostatic field strength was estimated to be 7×10^2 coul." But a coulomb expresses quantity of electricity, quite distinct from field strength per se. The latter should be expressed in terms of force (newtons) or electric intensity (newtons per coulomb) [J. A. Chalmers, *Atmospheric Electricity* (Pergamon, New York, 1957)]. As Chalmers stated, many authors refer to the electrical field of the atmosphere in terms of the potential gradient (volts per meter); the difference between the latter unit and electric intensity (*E*) is merely one of sign.

I am not clear on the meaning of the last two paragraphs of the report of Levengood and Shinkle. For example, in the sentence, "The electric field appears to provide a certain amount of protection and reduces the variations found outside the field"—variations in what? And in the sentence "Flies in the electric field are, in a sense, protected or shielded from external fluctuations," what external force is fluctuating? Are the authors referring in these two sentences to the natural, atmospheric electrical field? If they are, it seems to me that the field (potential gradient) within the laboratory building would not be important any-

(Continued on page 115)

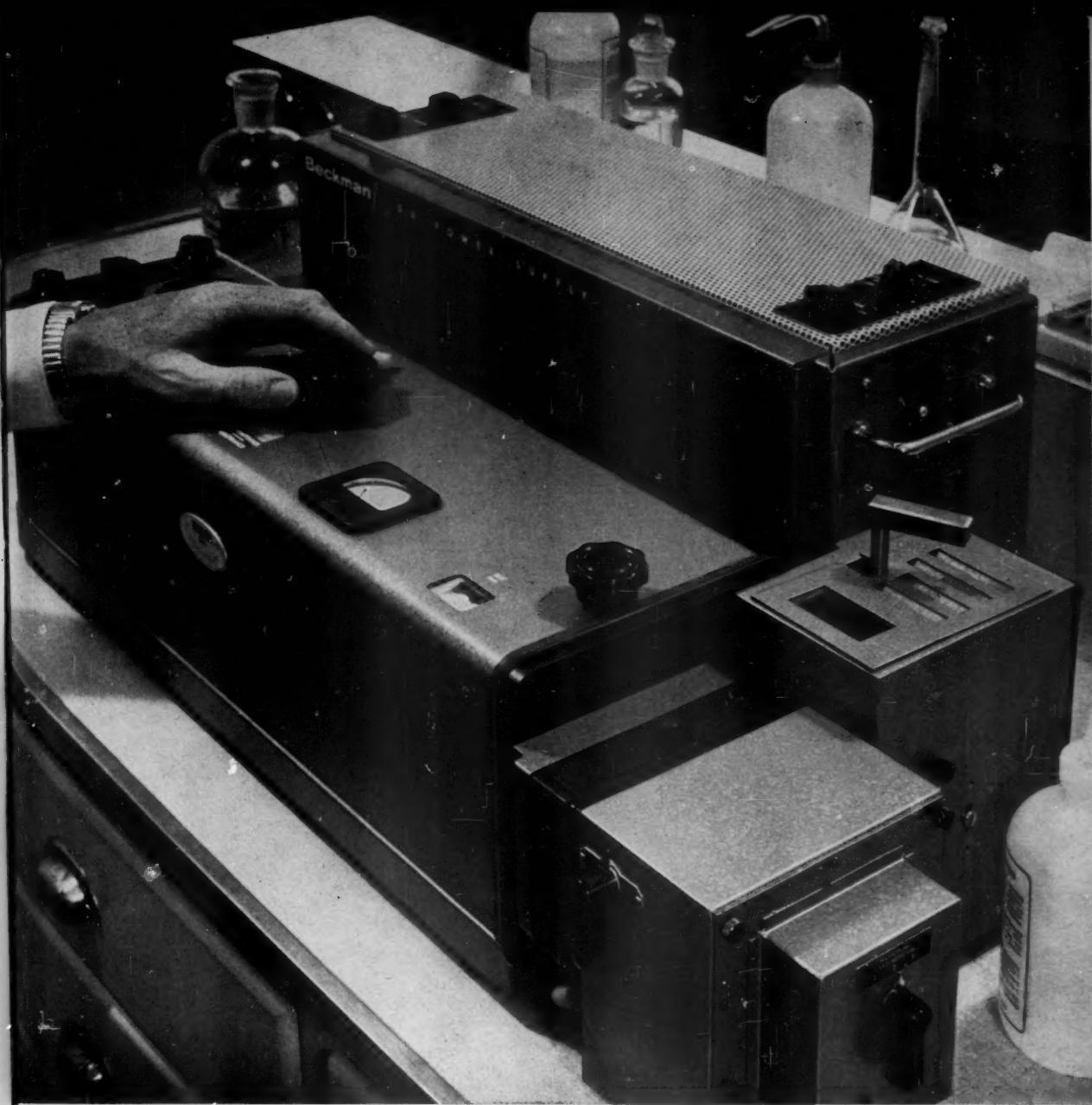


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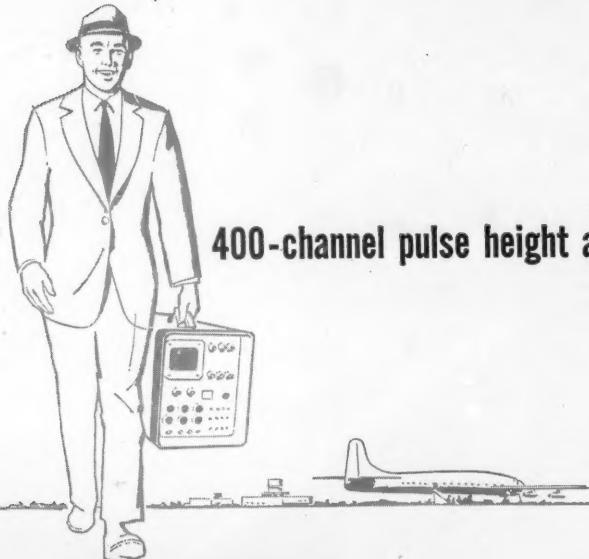
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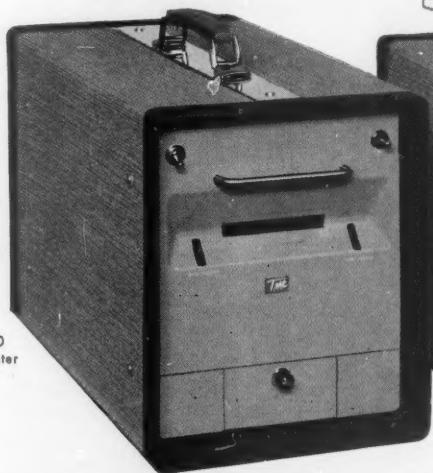
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A Strategy for Developing Talent

Our present policy for identifying and educating talented persons is, in good part, simply the mechanical product of our testing techniques, and there is good reason to question whether this policy is the best possible. Testing techniques, because of their use of certain statistical measures, tend to favor the broad scholar, the student with many interests and abilities. A more consciously directed program, however, might offer a different emphasis. This analysis and a new strategy based on it are given in Dael Wolfe's article on "Diversity of Talent" in *The American Psychologist*, August, 1960. The strategy is to increase our cultivation of persons who are not well-rounded, who are eccentric, one-sided, yet who, at least on that one side, are really superior. The claim is that such cultivation is valuable both from the viewpoint of the young persons whose future we guide and of the society in which they are to make their way.

There is nothing backhanded, according to the article, in the manner in which the broad, well-rounded student is favored by testing techniques. In selecting students for scholarships and fellowships and for admission to the next higher educational level, the present tendency is to use general measures of ability, to use the sum or the average of separate scores for separate types of ability, for example, rather than the separate scores directly. And there are good scientific reasons for this tendency. The use of sums or averages gives the best correlation between test scores and later achievement. Our measures of achievement in life, for the most part, are composites of several factors, and they are best predicted by tests that are also composites of several factors.

Good scientific grounds, however, may also be offered for not letting a concern with degree of correlation dominate talent development. For one thing, so the analysis continues, although different kinds of ability are often associated with one another, the association is far short of perfect. Some psychologists hold that a small number of primary abilities is sufficient to describe human ability, others find that a great number of special abilities is needed for this purpose, but all are agreed that ability is not a single, undivided trait. Another point about which psychologists are generally agreed is that an assortment of patterns of ability is consistent with achievement in a given profession. There has been a search for characteristic patterns of ability for various professions and it has failed. This means, according to the present analysis, that medicine, law, engineering, various branches of science, and other vocations all will profit by embracing diverse patterns of ability.

The strategy proposed, of course, does not call for slackening in the attention paid to the student who scores high everywhere. This student would get the same support he now gets, but attention would also be paid to the person who is exceptional in only one area. The strategy might mean poorer correlation between tests and subsequent achievement. The bet is, however, that it is a more productive way to increase the talent pool than simply to dip below the generally superior level, continuing to base awards on averages. In fact, the bet is that the strategy will enlarge the talent pool without loss in over-all quality. Students who, because of high gifts and intense interests along one line of endeavor, have neglected other lines would themselves no longer be neglected.—J.T.

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CURRENT PROBLEMS IN RESEARCH

Movement of Organic Substances in Trees

Photosynthates are translocated in a layer of bark only a fraction of a millimeter thick.

Martin H. Zimmermann

The development of land plants that began some 400 million years ago involved not only the problem of mechanical support of the plant body but also the problem of distribution of water, mineral nutrients, and photosynthetic products. Trees are among the most characteristic forms of land plants in this respect. The two centers of supply—namely, the roots that take up moisture and minerals from the soil and the leaves that carry out photosynthesis—are located at a distance from each other but are connected by the vascular system which consists of two "channels," the xylem and the phloem. In palms, as in other monocotyledonous plants, and in herbs, strands of xylem and phloem are joined into conducting bundles which are distributed in a ring or throughout the transverse-sectional area of the stem. In trees with secondary growth (increase in stem diameter), the two channels are in the form of cylinders, the wood (xylem) and the inner bark (phloem). The two systems conduct in essentially opposite directions: water and dissolved soil minerals move upward from roots to leaves in the xylem (transpiration stream), photosynthates move down from leaves to stem and roots in the phloem (assimilate stream). The movement toward growing centers, such as shoot tips, flowers, and young fruits, is unidirectional—that is, both streams move in the same direction.

The two systems enable the plant to distribute nutrients efficiently. Minerals that have been carried up by the trans-

piration stream into the leaves may be re-exported to other places via the assimilate stream. Metabolic energy is supplied through the phloem to roots in the form of carbohydrates, products of the photosynthetic activity of the leaves. These carbohydrates provide, at the same time, the carbon skeletons for nitrogenous compounds that are formed by nitrate reduction in the roots and that ascend via the xylem into the aerial parts (1, 2).

Accumulation of reserve materials such as starch is of considerable importance (3). A great percentage of the carbohydrates, exported from the leaves via the phloem, is deposited along the whole stem in bark and wood. These reserves are not only drawn upon for seed production during a seed year but are also utilized during the flush of growth in the spring. Some trees depend for their annual growth almost entirely upon the previous year's reserves (4). Hartig, one of the first forest botanists to give a detailed description of these matters, compared the awakening of a tree's activities after winter dormancy with the germination of a seed. All vital parts of the tree—leaves, shoots, root tips, and the conducting tissues interconnecting these organs—are renewed in the spring (5).

How substances move in the phloem is one of the oldest of botanical questions. Trees are very suitable objects for studies of this movement because they offer the unique advantage of great lengths of uniform translocation con-

ditions. However, working with trees has its difficulties. Large trees can hardly be grown under controlled conditions, and while most plant physiologists can transfer their material to a greenhouse or growth chamber, the tree physiologist has to take his experimental procedures to the trees outdoors.

Structural Organization of Bark

If we look at a transverse section of bark we can distinguish three structurally distinct parts that represent functionally different phases of bark tissue. The innermost and youngest layer, immediately adjacent to the vascular cambium, is the conducting phloem. As this layer is renewed by the cambium, the sieve tubes (the cell series in which conduction takes place) ordinarily collapse and the tissue enters its second phase, with the functional emphasis on storage. Still further out we can see the periderm, with the cork cambium producing cork cells—the outermost, dead bark which protects the living bark from desiccation and mechanical injury (6, 7).

In temperate regions, where distinct growth rings are present in the wood, growth rings can usually be distinguished in the phloem as well, although not as clearly as in the wood (8). Figure 1 shows a transverse section through the inner bark of white ash (*Fraxinus americana* L.). The innermost layer, adjacent to the cambium, is the 1960 phloem; successively older growth rings are outside (the 1959 and 1958 rings and a small portion of the 1957 ring are visible in Fig. 1). The most striking feature is that conduction usually takes place only through the latest growth ring of the bark, a layer some 0.2 millimeter thick.

In tropical trees there are usually no distinct annual growth rings. Renewal of phloem tissue and collapse of older sieve tubes, nevertheless, take place in a similar fashion. Figure 2 shows a transverse section through the inner bark of teak (*Tectona grandis* L.). The

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conducting layer is about 0.35 millimeter thick; the collapse of the sieve tubes in the older tissue is very conspicuous.

Significance of Callose

The sieve elements are long cells with end walls of a sievelike structure. In anatomical preparations the pores are often restricted with a substance, called callose, that stains in a characteristic way (9). Until recently, callose cylinders in the sieve pores have been regarded as standard features of conducting sieve tubes. It seems, however, that callose formation can occur very rapidly upon external mechanical or chemical stimulation (10). It was reported as early as 1886 that chemically fixed phloem tissue contains an abundance of callose, while pieces of bark that had been immediately killed by being dropped into boiling hot water contain only very small amounts (11). This has been confirmed in our own laboratory.

Even fresh sections may contain some callose, since the time that elapses between collecting the piece and staining the fresh section is often sufficient to allow callose formation. Whenever the phloem is injured—for instance, when a branch is cut from a tree or a piece of stem—great masses of callose are formed. This is an important sealing reaction of the plant (supplementing the instant sealing with "slime plugs" that takes place in many plants) that prevents excessive loss of translocated material upon injury. Just how much callose, if any, is present in normal, conducting sieve tubes remains to be investigated.

In addition to this formation of callose in the conducting layer of sieve tubes upon external stimulation, callose is formed naturally and regularly in autumn. In white ash this happens some two weeks after leaf abscission. The phloem remains essentially unchanged during the winter. About 1 May (in Petersham, Mass.) the buds begin to swell, the first early-wood ves-

sels appear in the wood, and the first sieve tubes are formed in the bark. Shoot extension as well as growth of xylem and phloem is essentially completed within the short period between 1 May and mid-June, at the expense of stored material. The callose in the previous year's phloem that had been formed in autumn disappears only slowly and incompletely (12). Occasional callose masses can be found in older phloem tissue. What causes these irregularities we do not yet know, but it is clear that the previous year's phloem can hardly be of any value for transport.

Methods for Physiological Experimentation

For physiological experimentation, trees, or parts thereof, are subjected to experimental conditions such as defoliation, girdling, and local chilling, and the effect of the treatment on translocation is studied. This can be done by paper-chromatographic analysis of sieve-

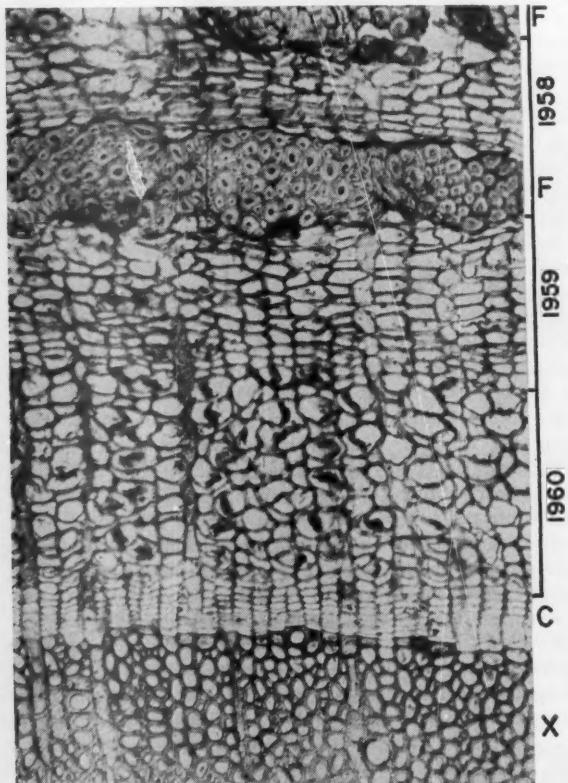
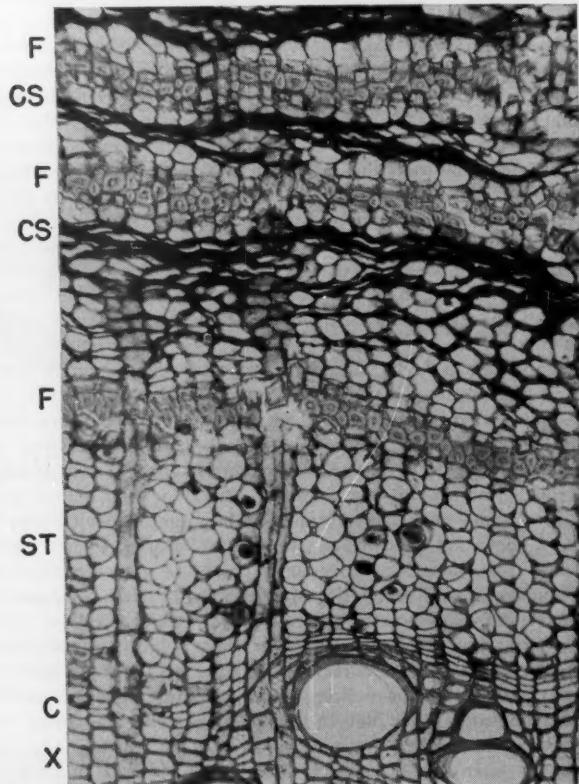


Fig. 1 (left). Transverse section through the inner bark of white ash (*Fraxinus americana* L.), showing growth rings. X, wood; C, vascular cambium; F, fibers ("sclerotic parenchyma cells") is technically more correct; they are formed in late June in 3-year-old phloem. Sectioned on 16 Aug. 1960 ($\times 165$). Fig. 2 (right). Transverse section through the inner bark of teak (*Tectona grandis* L.), showing conducting (ST) and collapsed (CS) sieve tubes. X, wood; C, vascular cambium, F, fibers ("true bark fibers," directly derived from cambium) ($\times 165$).



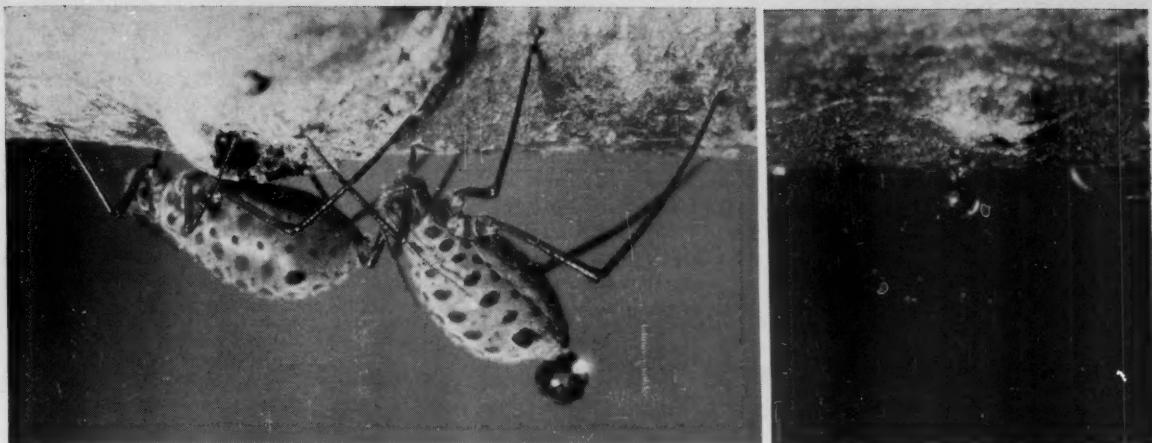


Fig. 3. (Left) Two aphids of the species *Longistigma caryae* (Harr.) feeding on the lower side of a linden branch. These fully grown individuals are approximately the size of a house fly. When feeding they release honeydew about once every 30 minutes. The actual stylets that penetrate the bark cannot be seen in the photograph because they are surrounded by the stylet sheath. (Right) Exudation from stylets after severance of the aphid.

tube exudate from various parts of the tree and comparison of the results with those from normal trees. This procedure is based upon the assumption that what we call "sieve-tube exudate" or "stylet exudate" is actually translocated material. Good evidence supports this assumption.

There are two methods for collecting sieve-tube exudate. The "classical" method was the first described by Hartig in 1860 (13). An incision is made into the inner bark, and the clear exudate can be collected with a graduated pipet. For quantitative studies, pipets of 5 cubic millimeters are most commonly used; occasionally pipets of smaller size are used, such as 2 or 4 cubic millimeters. Long before its usefulness in botany was discovered, the method was used for commercial production of sugar on a small scale. There are still regions in Sicily where certain *Fraxinus* species are cultivated for the collection of sieve-tube exudate. In the dry climate there, the exudate solidifies into small icicle-like "stalactites" (14). The anatomical location of such an incision is important. Exudate is obtained only if the cut reaches to the conducting part of the phloem (15). Too shallow a cut does not yield anything. Too deep a cut may cause the loss of the exudate to the xylem (the contents of which are usually under less than atmospheric pressure), especially in early summer; later on, the early-wood vessels are protected by a layer of heavily lignified late-wood fibers. Exudate can be obtained between about 1 July, when leaves have reached

maturity, and mid-October, when definitive callose is formed in the sieve tubes. This corresponds to the time when sieve tubes are functional and leaves are exporting photosynthates.

There are a number of organisms that have discovered the translocation channels as rich feeding grounds. They represent a great variety of both botanical and zoological orders, from fungi to flowering plants (such as *Orobanche* sp.) and from insects to birds (such as sapsuckers, *Sphyrapicus* sp.). Students of translocation have always taken great interest in these organisms (6). A most fascinating observation, made not long ago by entomologists, is that aphids do not suck but are being fed by the internal pressure of the sieve tubes: cut-off stylets from aphids which have been feeding continue to exude. This observation has been systematically developed into a method by Kennedy and Mittler, and by Mittler, with the willow aphid *Tuberolachnus salignus* (Gmelin) (16).

Figure 3 shows the North American aphid *Longistigma caryae* (Harr.) that feeds on linden, hickory, oak, and other trees (17). Colonies of parthenogenetically reproducing females live on the lower side of branches during the summer. They insert their stylets into a sieve element of the conducting phloem. So far we have sectioned some 40 blocks of bark containing stylets and have found that, whenever they exude, they reach the conducting phloem. The stylets tips are often disturbed or cut away by the microtome knife (the thinner the sections the greater the

chance of such disturbance), but in about one out of five cases the stylet tips can be found undisturbed within the section. One of these successful sections is illustrated in Fig. 4.

While aphids are feeding they eliminate surplus sugar by releasing honeydew (Fig. 3), which in nature is collected by ants and honeybees (18). When the stylet tips are properly placed in the phloem, as indicated by honeydew production, the insect can be cut from its mouth parts under anesthesia. If this is carefully done, exudation from the stylets continues, often for hours or even days (Fig. 3, right), and the exudate can be collected with a pipet. Exudation rates from *Longistigma* stylets are around 5 cubic millimeters per hour (19).

The stylet method does not replace Hartig's method; it is, rather, a valuable addition, having the advantage of operating with a minimum of disturbance and on a small scale. The exudate is the purest we can get, particularly suitable for the analysis of substances in low concentration (substances other than sugars). Furthermore, the stylet method can be used at times or with plants where Hartig's method does not yield sufficient material for analysis (20). Last but not least, it is in itself an interesting piece of evidence that the contents of sieve tubes are under pressure. The classical method, on the other hand, is most effective in experiments with large trees, such as those described below. A great number of samples can easily be taken within a short time.

Nature of Sieve-Tube Substances

Exudates of over 250 species, from 55 plant families, are known (2, 21). They contain 10 to 25 percent dry matter, of which 90 percent or more is sugar, occasionally accompanied by a sugar alcohol. Classified on the basis of sugar composition, three groups may be discerned, two extreme groups and a third with a large number of intermediate types. The first of these groups, represented by 50 known species of the

Leguminosae, contains sucrose as the only sugar. Exudates of the other extreme group contain large amounts of oligosaccharides of the raffinose type, which consist of sucrose and one or more D-galactose residues (Fig. 5).

Typical concentrations are, verbascose, 0.01M; stachyose, 0.25M; raffinose, 0.1M; sucrose, 0.1M. Families that belong to this group are the Bignoniacae, Oleaceae, and Verbenaceae. The Combretaceae, Myrtaceae, and Onagraceae are very close to this group.

The majority of other plant families are included in the third, intermediate group, with a high sucrose concentration (such as 0.5M) and variable, smaller amounts of oligosaccharides.

Hexoses are not present in most exudates and in the few species that do contain them, are present in very small amounts. They seem to play a very minor role in translocation.

Some sieve-tube exudates contain sugar alcohols in addition to sugars. Mannitol occurs in the Oleaceae and

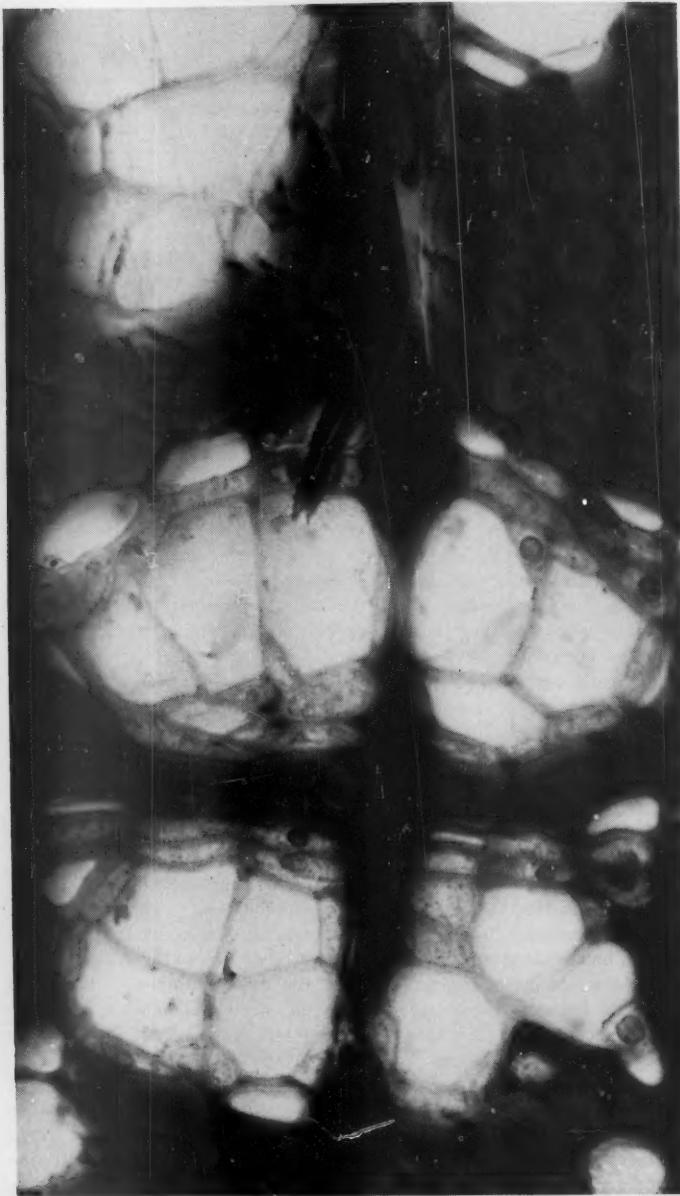


Fig. 4. (Left) Stylet tips in an individual sieve element. Two of the three tips are in focus ($\times 950$). (Right) The path of the stylets through the bark of linden (*Tilia americana* L.), shown in four subsequent transverse sections mounted to form the correct sequence. Photograph at top shows the stylet sheath, which is also shown in Fig. 3 ($\times 108$). The sections were made by Gerda Aerni.

possibly in some *Terminalia* species of the Combretaceae. Another sugar alcohol, tentatively identified as sorbitol, is found in some members of the Rosaceae, as well as sucrose. Typical concentrations are those in the sieve-tube exudate of *Prunus serotina* (Ehrh.): sucrose, 0.2M; sorbitol, 0.3M.

The concentrations of other substances (amino acids and so on) are only a small fraction of those of the carbohydrates; details of what is known about these have been reported in a recent review (2). The aphid stylet method will undoubtedly yield additional information in the future.

The concentration of samples, taken from a single incision, decreases continuously with time, so that the last sample, taken 30 minutes to 1 hour after the incision had been made, shows a concentration only 60 to 80 percent of that of the first one (22). This change in concentration, which becomes apparent in the exudate at the point of opening of the sieve tubes, extends within a short time over great longitudinal distances, but it cannot be detected in tangentially adjacent phloem (23).

The ease of longitudinal transport and the lack of tangential movement has been quantitatively demonstrated in still another way. Defoliation causes characteristic changes of the sieve-tube exudate, as described below. Defoliation of one half of a Y-shaped tree shows these changes to be very sharply defined all the way down the trunk (Fig. 6). There is a slight fanlike spreading of translocated material from the leafy side, but it follows an angle of less than 1 degree (23).

These results agree very nicely with phloem anatomy. The evolutionary trend is toward larger pores in the end walls (sieve plates), which may indicate increased conductibility (7). This emphasis on longitudinal translocation brings about a relative independence of the various sides of a tree. One side may die while the other side may survive for decades or even centuries. Or, in a tropical rain forest, one side of a tree may be dormant while another side is active.

The Mechanism of Translocation

The rate of phloem translocation is often extraordinarily high. According to Mason and Maskell it can be as much as 40,000 times the rate of sugar diffusion in water (24). The quest for the mechanism of this remarkable phenom-

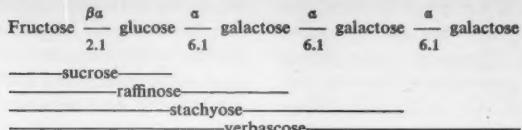


Fig. 5. The raffinose type of oligosaccharides.

enon has always greatly stimulated research. It appears to be clear that there are, in addition to rapid long-distance translocation in the phloem, other, usually much slower, types of transport in living tissues—movement in parenchyma cells (25), polar movement of auxins (26), secretion in nectaries (27).

Some workers regard long-distance movement in the phloem as a mass flow of solution; others visualize it as an ac-

tive process involving translocation of solutes without solvent (water). Bidulph and Cory have recently even claimed two different transport mechanisms in the phloem, but their evidence is far from convincing (28). Whoever has been able to observe the rapid and consistent exudation from cut-off aphid stylets cannot doubt that we are dealing with a mass movement of a solution.

Theoretically, of course, stylet exu-

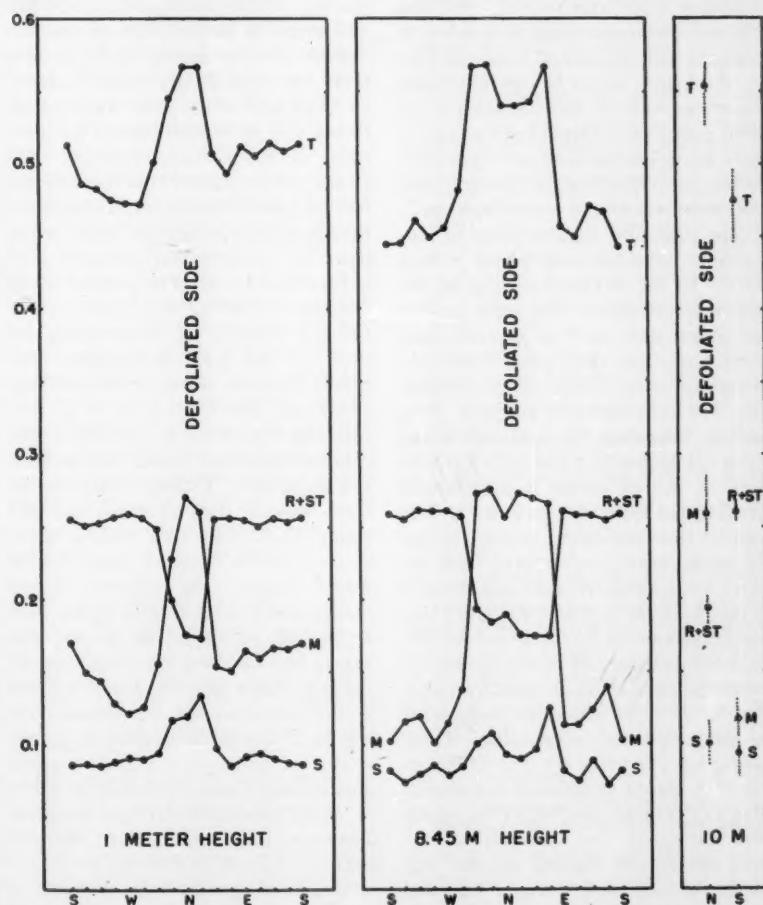


Fig. 6. Partial defoliation experiment. One half of the crown of a Y-shaped tree was defoliated. Seventeen days later samples were taken on each branch at a height of 10 meters, and around the trunk at both 8.45 meters (20 centimeters below the fork) and 1 meter. Molar concentrations are shown on the y-axis. S, sucrose; R + ST, raffinose plus stachyose; M, D-mannitol; T, total molar concentration. (From 23)

dation per se is not proof of mass flow in the intact plant, because the stylets are artifacts. However, experimental work, by its very nature, involves artifacts, and all our knowledge about translocation is based upon indirect evidence. Rejection of the concept of mass flow raises the difficult question of how a single sieve element 20 to 30 microns in diameter and 0.4 millimeter long can be continuously refilled three to ten times per second with a concentrated sugar solution without any visible injury.

Gage and Aronoff have recently rejected the concept of mass movement because they found it difficult to get tritium water to move out of leaves together with labeled photosynthates (29). This experiment is by its very nature a difficult one to carry out, although Bidulph and Cory have been successful (30). It must be extremely difficult to introduce water experimentally into the phloem, because in doing so one has to compete with the natural source of water, the xylem, which is very intimately associated with the phloem. Even if tritium water were introduced successfully, a heavy loss must be anticipated because of diffusional exchange with unlabeled water of the surroundings.

The sieve-tube vacuoles seem to represent a metabolic pool which is kept within by the semipermeability of the side-wall cytoplasm. The term *leaking* has often been used to describe how solutes get from sieve tubes to the surrounding tissue. This term is misleading. The sieve tubes do not leak. They remain turgescient for days and weeks after defoliation of a tree (31). Entry as well as exit of solute is a metabolic process that appears to be under remote control from the leaves. In the presence of leaves, sugars are removed from the sieve tubes along the stem and roots at a rate equaling that of sugar entry into the phloem in the leaves. When the tree is defoliated the rate of sugar removal from the sieve tubes decreases within a few hours. After about two weeks a net re-entry into the sieve tubes of the whole stem becomes apparent (31). Figure 6 shows the situation in a Y-shaped tree 17 days after one half of the crown was defoliated. The two sides of the tree are clearly defined all the way down the stem, and it can be seen that the exudate concentration on the defoliated side is even higher than that on the leafy side.

What is the mechanism of this solute entry into and exit from the sieve tubes?

We know very little about it. Defoliation experiments suggest that we are dealing with enzymatic processes. As soon as the source of solutes—the leaves—is missing, we find a rapid increase of sucrose at the expense of oligosaccharides. An α -galactosidase, therefore, must be one of these removal enzymes. This enzyme is not in the sieve-tube vacuole in solution; it is very probably attached to the sieve-tube cytoplasm and is in contact with the vacuole. There are two reasons for this conclusion: (i) the enzyme does not appear in the exudate; (ii) free galactose units do not appear in the exudate. Other enzymes, similarly placed, would be necessary to remove sucrose and mannitol (and all the other translocated substances) from the solution.

If we accept the idea that the sieve-tube vacuole is a metabolic pool, we still have to explain how this pool is moving. Münch postulated in 1930 that differences in turgor pressure are sufficiently great to account for mass flow (32). His original hypothesis included all living cells of the plant. Later on his theory had to be restricted to the sieve tubes of the phloem because it was found that substances are often secreted into the sieve tubes against a concentration gradient. Within the sieve tubes, however, concentration gradients have always been found to be positive in the direction of flow (22, 33). Trees are particularly suitable for such studies because of the great translocation distances through tissue under uniform conditions. The latter point is of considerable importance if one takes molar concentrations as a measure of probable turgor pressure. Tissues undergoing intense growth, such as shoot tips and young fruits, may well have a lower turgor than the exudate concentration would suggest. For example, if the phloem tissue is interrupted (if the stem is girdled) at one place on the tree trunk, growth along the stem between the live crown and the girdle will not be uniform any more, but instead there will be a very sharp increase of growth above the girdle. Concentration gradients in such a case can hardly be taken as direct indicators of turgor pressure. They are, indeed, difficult to interpret (23).

Studies of concentration gradients in trees in which several substances are being transported in major amounts are particularly interesting. In white ash the four substances stachyose, raffinose, sucrose, and mannitol make up the bulk

of the exudate. During the summer one invariably finds that the total molar concentrations decrease in the downward direction of the trunk. After leaf abscission in autumn or after artificial defoliation at any time during the summer, the concentration of all substances drops, some of the individual gradients (often stachyose and mannitol) becoming negative and the others remaining positive. The result is the complete disappearance of the total molar gradient, indicating cessation of translocation (22, 31).

The exudate concentration decreases some 0.01 mole per meter in the downward direction of a normal tree during the summer. According to Poiseulle's equation, the pressure gradient to which this would correspond is fully sufficient to force the solution through capillaries of the dimensions of the sieve-tube lumen and the combined sieve pores at the observed rates. The difficulty is that the sieve pores are not open but are filled with cytoplasm. These so-called connecting strands often appear to be fairly dense in electron micrographs (34), but one should not forget that this density is considerably increased by growth of callose cylinders after wounding. Freezing the stem section on the intact plant (without wounding) is absolutely essential if connecting strands are to be studied by electron microscopy. The question of how much resistance the connecting strand represents cannot be answered at present, nor is there any way to calculate it. The suggestion has been made that electroosmotic forces across the sieve plates may cause the solution to move (35). According to this theory the sieve plates would be not a passive resistance but the carrier of the electrical potential. Whatever may cause the solution to pass the sieve plates, we do know that it can pass easily. Mass flow of a solution is the only reasonable explanation for the refilling, three to ten times per second, of a sieve element with a highly concentrated sugar solution during hours and days of stylet exudation.

Evidence that exudate from bark incisions and aphid stylets is translocated material can be summarized as follows. Stylets, as well as incisions, whenever they are successfully placed, reach the conducting layer of the phloem. Living sieve tubes are the only continuous cell series that are unique for this layer. Stylet tips do end in a single sieve element, the cells that were identified as the channels of conduction by Schu-

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macher years ago (36). Furthermore, the time during which the exudate can be obtained corresponds with the time when sieve tubes are not restricted by callose. When branches of certain species such as hickory (*Carya sp.*) are cut from the tree and brought to the greenhouse, large masses of callose form on the sieve plates. Significantly, aphids are unable to feed on these, although they feed well on the intact plant.

The composition of these exudates corresponds to what has been deduced from tracer studies to be the translocated material (2). Lack of tangential transfer, as revealed by exudate analyses (23, 37), corresponds with results of tracer studies (38) as well as with phloem anatomy. The measurable concentration response to phloem injury over great distances within a short time is also an indication that we are dealing with the transport system (23). The sharp drop in exudate concentration after defoliation corresponds with the estimated rate of turnover in the phloem (31). Pentoses, applied in agar blocks to the inner bark, are taken up and translocated downward and can be found in exudate there (39). During natural as well as artificially induced leaf senescence (yellowing), relatively large amounts of nitrogenous and phosphorus compounds are exported from the leaves (36). At such times these substances appear (or if already present, increase) in the exudate from stylets as well as from incisions (16, 22, 37, 39). Indeed, the list of evidence could be extended almost indefinitely.

Present and Future Problems

What are the present and future problems in research on translocation in trees? One of the most important aspects is the entry of solutes into, and their removal from, the sieve tubes. How do these mechanisms operate and what are the factors that control them? Of general physiological importance is the finding that sugar alcohols are translocated in certain plant families. Mannitol is often used as a metabolically inert substance for physiological experimentation. One has to be very careful

because some plants do utilize sugar alcohols. In addition, the plant's ability to use them may be dependent on the presence or absence of the leaves.

At present, studies are in progress concerning the effect of locally applied temperature on translocation. The problem is complicated by the fact that any applied temperature is carried upward with the transpiration stream. For example, if the tree is chilled at a height of 5 meters, the cooled xylem water lowers the temperature all along the stem above 5 meters; the chilling effect thus remains by no means a local one. To bring about the desired condition, a heating tape above the place of chilling has to warm up the xylem water to normal temperature. Temperatures are measured on a multiple thermistor thermometer with insulated surface probes placed near the conducting phloem, and the voltage of the tape is adjusted accordingly.

From the viewpoint of practical forestry, phloem transport has an importance of which few foresters are aware. Most material that tree utilizes for its growth is manufactured in the leaves. From there it moves downward through the conducting phloem, a layer only a fraction of a millimeter thick. Leaves as well as the vascular system are rebuilt every spring, beginning with the time of bud burst. For biology as a whole, the study of complete organisms is a necessary complement to cellular physiology, which has become increasingly important during the past decades. There are many problems that can be solved with plant parts or with seedlings in the greenhouse, but there are others that can only be solved with large and intact plants. Trees are ideal for such studies and will undoubtedly yield valuable information in the future.

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To Honor Fechner and Repeal His Law

A power function, not a log function, describes the operating characteristic of a sensory system.

S. S. Stevens

One hundred years ago G. T. Fechner (1) published the fruits and findings of a ten-year labor—an event that we celebrate as the nascence of the discipline called psychophysics. In the century since the *Elemente der Psychophysik* first made its stir, the simple but controversial logarithmic law that goes by Fechner's name has invaded almost all the textbooks that mention human reactions to stimuli. It is fitting and proper, therefore, that we should gather this year in a symposium to mark the anniversary of these beginnings and to inquire how the issues stand in 1960.

Perhaps the most insistent question on this 100th anniversary of Fechner's monumental opus is how its author could have known so much and have made such a wrong guess. (He believed that, unlike errors in general, errors in perception are independent of the perceived magnitude.) Talent, erudition, originality—each of these gifts was his in generous measure, and he applied his skills with signal success to several different domains. Not only did he create psychophysics and pioneer in experimental esthetics, but he also laid the foundations for what von Mises (2) later transformed into a well-known theory of probability based on the concept of a "collective."

But it was Fechner's version of the psychophysical law that really made him famous. With it he founded psychophysics and sent it off on a curious tangent—a deflection that lasted for the better part of a century. If we regard a hundred years as a long time—and it certainly seems long in the fast-moving evolution of modern science—then Fechner was almost right in his defiant forecast of 1877 (3): "The Tower of Babel was never finished because the workers could not reach an

understanding on how they should build it; my psychophysical edifice will stand because the workers will never agree on how to tear it down."

These words were published 17 years after the appearance of the *Elemente*. By that time Fechner had had full opportunity to correct his magnum error, for at least two different arguments had by then been made in favor of what has more recently appeared to be the correct relation between the apparent magnitude of a sensation and the stimulus that causes it.

1) Brentano (4) had suggested that Weber's law may hold at both levels: stimulus ϕ and sensation ψ . In other words, $\Delta\phi = k\phi$, and $\Delta\psi = k\psi$. This in itself is not truth, but a simple, if illegitimate, Fechnerian integration leads from these two equations directly to the correct general form of the psychophysical law. It was Fechner himself who argued that Brentano's suggestion had to be wrong, because it would entail a power-function relation between stimulus and sensation.

2) Plateau (5) had suggested that, when we vary the illumination on a scene made up of different shades of gray, it is not the subjective *differences* but the subjective *ratios* that remain constant. Plateau therefore conjectured that the psychophysical relation might be a power function—a conjecture that he later renounced for a wrong reason.

Because both these suggestions led to a power law, as opposed to a logarithmic law, Fechner was inspired to write long and bitterly in his denunciation of them. It was asking too much, perhaps, to expect a professor to change his mind after two decades of devotion to an ingenious theory. Moreover, by dint of his industry and his polemics, Fechner succeeded in making the

logarithmic function the sole contender, so that little or nothing was heard of the power function for many decades. If a change is now setting in, it is because new techniques have made it plain that on some two dozen sensory continua the subjective magnitude grows as a power function of the stimulus magnitude (6).

It is understandable that Fechner should fight stubbornly throughout his later decades to salvage his intellectual investment in the thesis that a measure of the uncertainty or variability in a sensory discrimination can be used as a unit for the scaling of the psychological continuum. He had sensed the essence of this possibility as he lay abed on that famous morning of 22 October 1850, and he had put the idea promptly and tenaciously to work. But why should such an unlikely notion have persisted for so long in other circles, and why should it have blossomed out in such noted and provocative guises as those devised by Thurstone and his school? I have puzzled so often about the ability of this fancy to persist and grow famous that I have accumulated a list of possible reasons for it. I will run quickly through a few of them, not because they are the true causes, or are exhaustive of the possibilities, but only because listing them may inspire others to inquire further into this anomaly in the history of scientific thought.

Excuses for Unitizing Error

These, then, are some of the excuses we might offer for the popularity of methods that try to create measurement by unitizing the residual noise in a stimulus-response sequence of one kind or another.

No competition. There are (or were) no competing methods. Plateau, to be sure, had asked eight artists each to paint a gray that appeared to lie midway between black and white, but this method of bisection did not catch on in a way that could challenge the arsenal of procedures aimed by Fechner at the determination of the *jnd* (just noticeable difference). When a bad theory is punctured it does not shrink quietly away. It retires from the field of sci-

The author is director of the Psychological Laboratories, Harvard University, Cambridge, Mass. This article is adapted from the text of an address delivered at a centennial symposium honoring Fechner, sponsored by the American Psychological Association and the Psychometric Society, which was held in Chicago in September 1960.

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entific contest only when pushed aside by a stronger theory. Plateau shot his perceptive shaft into the Fechnerian blimp by asserting that his bisection results entailed a power law rather than a logarithmic law, but he then turned the experimental attack over to his friend Delboeuf (7), who, for reasons not entirely clear, proceeded to obtain bisection data that approximated the Fechnerian logarithmic prediction. Plateau thereupon reversed his view and turned to other topics, leaving Fechner to patch up his defenses and carry the day. If Plateau, whom we remember well for his part in establishing the Talbot-Plateau law relating to the brightness of intermittent light, had fought back at this juncture instead of capitulating, the story might then have taken a happier turn. Plateau's method of bisection may not be the best of procedures, but it could have carried him a long way toward the establishment of the correct psychophysical law. If that had happened, psychophysics would have been spared a hundred years of futility.

At this point let me interrupt the story and try to justify my belief that a man of Plateau's great talent could have made an effective stand against Fechner by perfecting the method of bisection and using it to produce a sufficient body of experimental results. It is true that the method of bisection, like all methods that attempt to partition the distance between a pair of stimuli, contains systematic biases that prevent its generating a linear segment of the scale of sensation. Applied to sensory intensity—the so-called prototypic continua—these methods produce "partition scales" whose curvature is always downward when they are plotted against the ratio scale of subjective magnitude (8). Nevertheless, as Plateau had surmised, these procedures are capable of verifying the power law. The estimate they give of the exponent is generally too small, but often the discrepancy is only 10 to 15 percent.

A series of brightness bisections that R. J. Herrnstein and I obtained in 1953 are shown in Fig. 1. We were wondering whether brightness bisections would exhibit the hysteresis that manifests itself in other sense modalities when the order of presentation of the stimuli is reversed. The answer is yes (9), but more important to our immediate concern are three other features of the outcome. (i) The curvature of the functions in the semi-log plot of Fig. 1 demonstrates that the

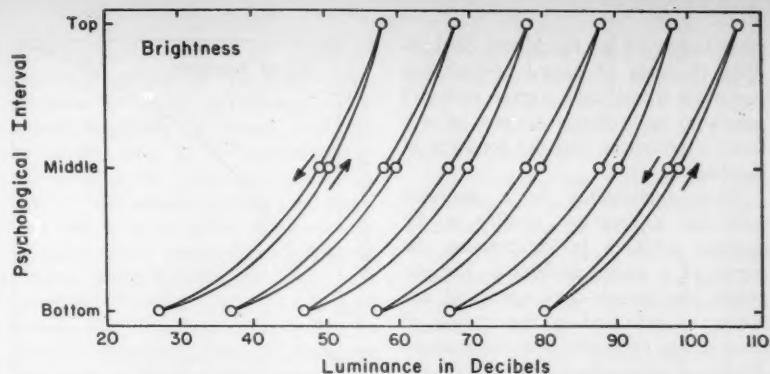


Fig. 1. Bisections of brightness intervals, showing the hysteresis effect produced by the order of stimulus presentation. The abscissa shows the luminances in decibels relative to 10^{-10} lambert. The apparent midpoint is at a higher luminance when the luminances are viewed in ascending order. Each bisection was made by from 14 to 19 observers, each of whom repeated the task three times, in both ascending and descending order. Thus, each point is based on 45 to 57 bisections. The bisection values obtained by averaging the ascending and descending series are consistent with a power function having an exponent slightly under 0.3.

relation between sensation and stimulus is not logarithmic—that is, the bisection does not occur at the geometric mean. (ii) The similarity in the curvature obtained over a wide range of absolute stimulus levels is consistent with the power function. (iii) The bisection values obtained by averaging the ascending and descending series determine that the power function has an exponent of about 0.3. This compares with the slightly larger value, 0.33, typically found in experiments on the ratio scaling of the brightness of luminous targets seen in the dark.

Admittedly, modern apparatus makes the experiment easier now, but it is difficult to see how any ghost other than the *Zeitgeist* could have prevented the establishment of the power law back in the last century if Plateau had followed through. Merkel (10), incidentally, had the necessary apparatus all set up in 1888, and his limited results on bisections agree well with those in Fig. 1. But no one paid much attention to Merkel. He is the one who gave us the method of ratio production, "stimulus doubling," as he called it. With this new procedure he was fully equipped to rescue psychophysics, but neither he nor anyone else seemed to know it. Oh, to have the perceptual resolving power of hindsight!

The universality of variability. The unitizing of "noise" is attractive because confusion and variability, like death and taxes, are always with us. Normally a nuisance to science, dispersion among people's judgments becomes grist for the mill when Fechner

makes dispersion into a "difference limen" and calls it the unit of his scale; and it keeps the mill wheel whirling when Thurstone enters dispersion into his "equation of comparative judgment" and computes scale values for the stimuli. While preserving the "logic," as he called it, of Fechner's procedure, Thurstone ventured to "extend the psychophysical methods to interesting stimuli" (11). Among other spirited questions, he asked, "which of two nationalities would you prefer to marry?" which to some people is a livelier issue than any possible question about lifted weights.

Here I must digress to modify the foregoing assertion regarding the universal presence of confusion and variability. Dispersion among data is always with us, to be sure, but sometimes it is not there in sufficient bulk for the smooth working of the Thurstonian transformations. Mischief as it may seem, investigators who use these methods sometimes feel called upon to seek noisier data than those they happen to have gathered. The premium normally placed upon precision, repeatability, and the elimination of perturbations may turn into a liability when the perturbations themselves provide the alleged unit of measurement.

Wide applicability. The universality of noise imparts an equal universality to the Fechner-Thurstone scales. They can be erected with and without the knowledge of an underlying stimulus metric. Fechner scaled subjective weight by *jnd*'s and determined esthetic judgment by paired comparisons. Thur-

stone improved the machinery for handling the data of paired comparisons and made it applicable (given sufficient noise) to such elusive matters as attitudes, preferences, and the goodness of handwriting.

Wide applicability is a desirable trait, but it does not qualify as the decisive attribute in determining the merits of a scaling technique. For example, the operation of empirical addition, as practiced in the scaling of some of the "fundamental" magnitudes of physics, especially length and weight, has a very limited applicability but an overwhelming importance. One is led to suspect that universality may be less important than it seems at first sight.

It should be remarked that some of the modern psychophysical techniques—magnitude estimation, for example—can also be applied to stimuli for which there is no underlying metric. We have recently scaled the apparent roughness of sandpapers on both a ratio and a category scale and have demonstrated that roughness as felt by the finger is a prothetic continuum—all this despite our having no metric scale of the stimulus involved. In order to erect a psychological ratio scale, the experimenter needs only a nominal scale of his stimuli—he must be able to keep track of which is which.

The experiment on roughness was carried out by Judith Rich and Irma Silverman as a laboratory exercise. They presented 12 grades of sandpaper (nominally 24 to 320 grit) to the observer, who made two sweeps with his first and second fingers over each paper. The papers were presented twice each, in an irregular order, to each of 12 observers. For the magnitude estimations, a paper of medium roughness was presented first and called 10. The observer then assigned numbers proportional to the apparent roughness of each of the sandpapers as he felt it. For the category judgments, the smoothest paper was presented and called 1, and the roughest was presented and called 7. The observer then judged each paper twice, in irregular order, on a 7-point scale. Again there were 12 observers.

As shown in Fig. 2, the partition scale that results when the observers try to divide roughness into seven equally spaced categories is nonlinearly related to the scale erected by direct magnitude estimation. This outcome is the standard finding on prothetic continua. It is as though the observer,

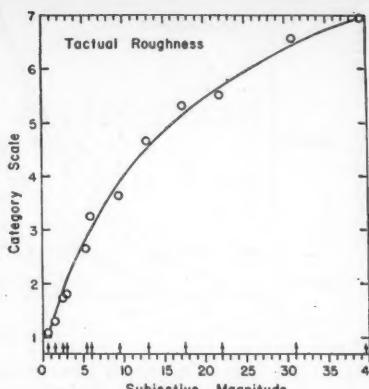


Fig. 2. Category and ratio judgments of the roughness of sandpaper. The arithmetic means of the category judgments (ordinate) are plotted against the scale derived from the geometric means of the magnitude estimations (abscissa). The marks along the abscissa show the locations of the sandpaper stimuli on the linear scale of subjective roughness.

when he tries to partition a prothetic continuum into equal intervals, finds himself biased by the fact that a given difference at the low end of the scale is more noticeable and impressive than the same difference at the high end of the scale. This asymmetry is not present on metathetic continua (for example, pitch and apparent azimuth), and there the category scale is not systematically curved.

The scaling of roughness should make it plain that the methods used in "the new psychophysics" are not restricted to psychological continua for which a stimulus metric is known at the level of an interval or a ratio scale—the scales that we commonly call quantitative (12). If further evidence is needed, look, for example, at Ekman's application of ratio scaling procedures to the esthetic value of handwriting (13). He cites evidence that the Thurstonian scale is nonlinearly related to the ratio scale in the way that one would expect for a prothetic continuum. Ekman clings to the view that there is still a use for the "indirect" methods that unitize confusion, because there may be psychological variables that cannot be directly observed. Whatever turn this particular argument may take, there is no denying that the challenge is out. Is there any substantive problem relating to the assessment of a subjective variable whose solution cannot be reached by direct ratio scaling procedures?

Easy on the observer. An argument for the variability methods is that they place minimal requirements on the person making the judgment. His task is easy; all he needs to be is variable. As a more specific instance of the argument for the use of a measure of dispersion as the unit of the subjective scale, we find Garner (14) saying that these methods are "more legitimate, valid, and meaningful for the scaling of loudness than are those methods which make use of various types of direct response on the part of the observer." Another specific merit claimed for these "discriminability" procedures is the stability of the results.

If it were true (and this we may well dispute) that *ind* scales show more stability than ratio scales of subjective magnitude, we could still with justice ask that rude but pointed question: So what? It is conceivable that the noise ordinarily encountered when the observer responds to pairs of stimuli is much the same in magnitude from experiment to experiment, but so also may be the observer's temperature.

Relevance may be more crucial than precision.

Model-maker's delight. Scaling by the unitizing of variability has the further advantage that it poses a challenge to our ingenuity and allows us to invoke elegant formal models in one or another aspect of the enterprise. In this respect it contrasts with those prosaic procedures of measurement that offer only the computation of a median or a geometric mean as the terminal ritual. As soon as it is decided that a measure of dispersion can be used for something more than the measurement of dispersion, new vistas open, and the model builders proceed to devise ingenious exercises in matters ranging from axiomatics to the programming of computers. All this ferment is interesting and good. It would be even better if it stood on some firmer base than disagreement among human judgments.

At the risk of giving aid and comfort to the Thurstonians, I have elsewhere (15) suggested that a closer approximation to the correct scale of subjective magnitude on a prothetic continuum would be achieved if it were assumed that discriminable (that is, subjective) dispersion is proportional to the psychological magnitude. The subjective dispersion is usually assumed to be constant, an assumption known as the Thurstonian "case V," which is

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essentially the same as the Fechnerian assumption that each *jnd* unit represents the same subjective difference on the psychological continuum. Fortunately for a sane approach to psychological measurement, the proportionality assumption, which I have suggested might be called "case VI" (16), turns out to have two defects. First, it is not quite true that variability in the subjective response is always proportional to subjective magnitude, and second, even if this were a good assumption, the resulting Thurstonian scale would turn out to be only a logarithmic interval scale. Psychological values separated by equal units of dispersion would not stand equidistant from one another; they would stand in a constant ratio to one another. But no one seems yet to have discovered an interesting use for a logarithmic interval scale—except to christen and describe it, and to point out that it remains invariant under the power group of transformations (12).

Prominent among the models contrived for the mirroring of human variability is the development known as detection theory (see 17). Here there seems to be no thought of measuring sensations or other subjective magnitudes; the question at issue is the narrow problem of the rules that govern the ability of a person to detect a signal immersed in a noise. Since analogous problems have been addressed by engineers and mathematicians in other domains, an elaborate model incorporating certain aspects of statistical decision theory stands ready for application to the psychophysical case. Actually, the model is perhaps not quite so ready as I seem to imply, for arguments are in progress regarding the resemblance between the nature of the noise assumed by the model and the properties of the noise that might reasonably be assumed to reside in the observer (see 18). In any case, we have here an instance in which the contentions regarding the model and its applicability serve to generate a sparkling interest in a problem that has little substantive flesh on its bones.

Pseudo differential equations. In some quarters the popularity of the methods that try to create measurement out of variability has been sustained by a misidentification. The standard deviations and quartile points of frequency distributions of judgments have become identified with differentials. The custom is to write ΔI when

what we really mean is the scatter of some dial settings or the relative frequencies of some confusions. It is argued implicitly by some, explicitly by others, that ΔI is determined by the slope of the "operating characteristic" of the sensory system and therefore that it is as useful to psychology as differential equations are useful to physics. Here is what we read in a paper by Nutting (19). "As is well known, the visual *sensation* cannot be directly measured, but its derivative *sensibility* is readily measurable, and from this the sensation may be readily deduced just as the scale of an ammeter may be readily reconstructed if the sensibility is known for all currents."

Admittedly, that was an engineer talking, but I wish I could say that no modern psychophysicist holds similar views. Many recent arguments have turned on the question of whether it is possible for the psychophysical power function to vary in steepness (exponent) without a concomitant change in the resolving power. Some people claim to enjoy a compelling intuition to the effect that, when the psychophysical function gets steeper, the resolving power must get better. This, I suppose, is a tacit admission that the person in question has not made the proper distinction between a differential and a standard deviation.

Let us instruct ourselves on this issue by a brief sample of facts. The sensation of electric shock grows rapidly with intensity (exponent = 3.5), but the resolving power on this continuum seems to be little if at all better than in the vibration sense, where the exponent is several times smaller (20). The measured resolving power for differences in brightness changes by about 50-fold when the area of the stimulus changes from a smallish point to a wide field (see Hunt, 21), but no comparable change occurs in the exponent of the brightness function (9). Actually, the brightness function is slightly steeper for point sources (exponent = 0.5) than for larger targets (exponent = 0.33), but the difference would be in the opposite direction if a smaller *jnd* went with a steeper function. In the auditory sense modality the *jnd* for intensity can be altered merely by changing the frequency of the tone. As Newman (22) pointed out, the difference in the resolving power at 80 and at 1000 cycles per second is such that the subjective size of the *jnd* cannot possibly be constant for all pure tones.

These examples are just a few of the more dramatic instances in which variability, in the guise of the *jnd*, has failed to behave as a proper unit of subjective measurement.

Nevertheless, the number of man-hours devoted each year to the measurement of one or another aspect of sensitivity, sensibility, resolving power, detectability, or just plain *jnd* attests a persistent belief in the utility of these measures. By and large, they are remarkably tedious measurements; yet the cumulative curves in which they are displayed merely gauge the noise that happens to characterize the system under the circumstance chosen for the experiment. If the noise could somehow be reduced, the measured variability would grow smaller, but there is no reason to expect that the subjective magnitude, represented by some average value of the distribution, would be thereby altered. It is not required that the mean and the standard deviation vary together.

If so much trouble has resulted from the identification of ΔI with a measure of the slope of the magnitude function, a simple remedy suggests itself. Perhaps, if we stop writing ΔI when we mean a measure of dispersion on a frequency distribution, the confusion may get itself cleared up in a generation or two. The most common measure of resolving power in sensory psychophysics is the median deviation—the 75-percent point on the cumulative "psychometric function" relating proportion of correct judgments to stimulus difference. Since this value is sometimes called the quartile point, why not replace the abbreviation ΔI by the abbreviation Q ? We could then write the general linear form of Weber's law as $Q = k(I + I_0)$, and in the process we would discourage the view that Q 's are something that can profitably be added up, or that can be regarded as indicative of the slope of the function relating sensation to stimulus.

The Question of the Neural Quantum

The ubiquitous variability of the human response has not only provided a tempting basis on which to build a deceptive theory of psychological measurement, it has also obscured the inner workings of the discriminatory mechanism. Under most procedures used to measure the *jnd*, some variety of noise sets bounds on the observer's resolving

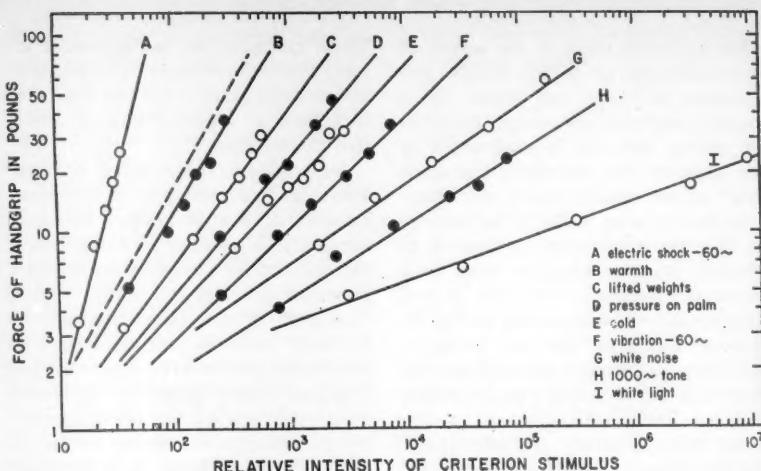


Fig. 3. Nine equal-sensation functions, obtained by matching force of handgrip to various criterion stimuli. The relative positioning of the functions along the abscissa is arbitrary. The dashed line shows a slope of 1.0 in these coordinates. Each point stands for the median force exerted to match a criterion stimulus. Ten or more observers participated in each of the nine experiments.

power and thereby determines the measured size of the resulting error distribution—the Q . What would happen if, by some contrivance, we could rid the experiment of noise, or at least of much of it? No doubt, if sufficient "quiet" could be achieved, the "grain" and discontinuities in the action of the sensory system would begin to manifest themselves, for it is highly unlikely that the neural processes that mediate discrimination are devoid of some all-or-none property. As a matter of fact, direct evidence for an all-or-none step function in the action of sensory discrimination has been observed in several experiments that have been undertaken to perfect the experimental arrangement to the point of a sufficient suppression of variability (for a recent review, see 23). The success of some of the experiments that have sought evidence for a "neural quantum" (NQ) suggests that valuable findings may attend the suppression of noise and variability. It may prove better to struggle for the reduction of uncertainty than to try to enshrine it as the measure of psychological magnitudes.

The Psychophysical Law

All the evidence that was heaped together in previous decades for the purpose of refuting the Fechnerian dogma made no great hole in the "psychophysical edifice." Even the eloquent ridicule by William James, who went

so far as to mix his metaphor in "striking Fechner's theories hip and thigh and leaving not a stick of them standing," could do little to change the content of the textbook discussions of "Fechner's Law." The task of clearing

Table 1. Representative exponents of the power functions relating psychological magnitude to stimulus magnitude on prototypic continua.

Continuum	Exponent	Conditions
Loudness	0.6	Binaural
Loudness	0.54	Monaural
Brightness	0.33	5° Target, dark-adapted eye
Brightness	0.5	Point source, dark-adapted eye
Lightness	1.2	Reflectance of gray papers
Smell	0.55	Coffee odor
Smell	0.6	Heptane
Taste	0.8	Saccharine
Taste	1.3	Sucrose
Taste	1.3	Salt
Temperature	1.0	Cold, on arm
Temperature	1.6	Warm, on arm
Vibration	0.95	60 cy/sec, on finger
Vibration	0.6	250 cy/sec, on finger
Duration	1.1	White-noise stimulus
Repetition rate	1.0	Light, sound, touch, shocks
Finger span	1.3	Thickness of wood blocks
Pressure on palm	1.1	Static force on skin
Heaviness	1.45	Lifted weights
Force of handgrip	1.7	Precision hand dynamometer
Autophonic response	1.1	Sound pressure of vocalization
Electric shock	3.5	60 cy/sec through fingers

the scientific bench top of the century-long preoccupation with the *jnd*, and the consequent belief in logarithmic functions, demands the cleansing power of a superior replacement. My optimism on this score has been recorded in other places, but I would like here to suggest that, if I seem to feel a measure of enthusiasm for the power law relating sensation magnitude to stimulus intensity, it is only because that law seems to me to exhibit some highly desirable features. Not the least of these desiderata is its apparent generality. On more than a score of sensory continua, the subjective magnitude ψ has been shown to grow as the stimulus magnitude ϕ raised to a power n . More specifically,

$$\psi = k(\phi - \phi_0)^n$$

where ϕ_0 is the effective threshold. As yet we have encountered no exception to this rule. Some of the exponents, a few of them not yet very firmly determined, are listed in Table 1. If the psychophysicists complete the sweep of the sensory domain and recover power functions at every turn, we may anticipate that little room will remain for Fechner's logarithmic relation. Perhaps Luce's (24) penetrating analysis has left no room for it anyhow.

William James (25) once addressed himself to Fechner's *Massformel*, $S = c \log R$, by pulling himself up to a towering indignation and letting go with: "No human being, in any investigation into which sensations entered, has ever used the numbers computed in this or any other way in order to test a theory or reach a new result."

Whether James was precisely correct in this stricture is beside the point. What he was invoking was the pragmatic test by which all scientific principles must be judged—including my own candidate, the power law. That brings us to the pay-off question: In what ways has the new psychophysical law done better than the old one in serving a scientific purpose?

Applications and Validations

As positive answers to this question, four examples can be marshaled, each concerned with a different asset of the new approach to psychophysics.

1) We have already noted that the power function governing the growth of subjective brightness (exponent = 0.33) predicts, with only a minor systematic error, the behavior of an ob-

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server who undertakes to bisect the interval between two levels of luminance (see Fig. 1). A similar story can be told for loudness (26). One success of the new psychophysics has been the pulling together of many loose ends in a way that discloses consistencies and uniformities where none were apparent before. The nature of the biases in partition scales and the relation of these scales to ratio scales are fast becoming clear. Not only that, but the relation between subjective magnitude and the scale that is generated by Thurstone's method of successive intervals, which "unitizes" the confusions among category judgments, has yielded to orderly analysis by Galanter and Messick (27). Since the noise and confusion in judgments of loudness tend to grow in direct proportion to the subjective magnitude, it is not surprising that the confusion scale generated by discarding the mean and processing the variability turns out to resemble a logarithmic transform of the ratio scale of loudness.

2) In some ways the most dramatic validation of the scales generated by asking observers to make numerical estimations of sensory intensity is the demonstration that these same scales can be generated even if no appeal is made to "number behavior" at all. By means of cross-modality comparisons, each subjective continuum can be related to each other continuum, and, for the critic who thinks he will feel better if all reference to numerical judgments is avoided, the family of power functions governing the various sensory continua can all be assigned their appropriate exponents relative to that of some "base continuum," such as apparent length of lines. In practice, of course, we have been content to go along with results of the several procedures involving numerical methods, because these findings have stood the test of cross-modality validation. The argument runs as follows.

If, given an appropriate choice of units, two modalities are governed by the equations

$$\psi_1 = \phi_1^m$$

$$\psi_2 = \phi_2^n$$

and if the subjective values ψ_1 and ψ_2 are equated by asking the observer to make the one sensation seem as strong as the other at various levels, then the resulting equal-sensation function will be given by

$$\phi_1^m = \phi_2^n$$

In terms of logarithms

$$\log \phi_1 = \frac{n}{m} (\log \phi_2)$$

In log-log coordinates, therefore, the equal sensation function should be a straight line with a slope equal to the ratio of the two exponents.

This prediction was nicely borne out by a series of cross-modality matches between all possible pairs of the three continua, loudness, vibration on the finger tip, and electric shock to the fingers (20). From this encouraging beginning, the procedure of cross-modality matching has been extended to numerous other pairs, with special emphasis on what might be called scaling by squeezing.

Using a precision dynamometer, J. C. Stevens and Mack (28) worked out the subjective scale relating the apparent force of handgrip to the physical

force exerted by the subject. This relation turned out to be a power function with an exponent of 1.7. Equipped with this scale, we then proceeded to take the measure of other sensory continua by asking observers to squeeze the dynamometer until the sensation of strain matched the apparent intensity of a criterion sensation in some other modality (29). A sample of the results is shown in Fig. 3, where two important facts stand out. All the data approximate straight lines in the log-log plot, and the slopes stand in the same order as the respective exponents listed in Table 1. Less obvious but even more crucial are the exact values of the slopes in Fig. 3. If these values are multiplied by the factor 1.7, the products agree reasonably closely with the values of the exponents listed in Table 1 (6).

In another investigation, the cross-modality comparison of loudness and

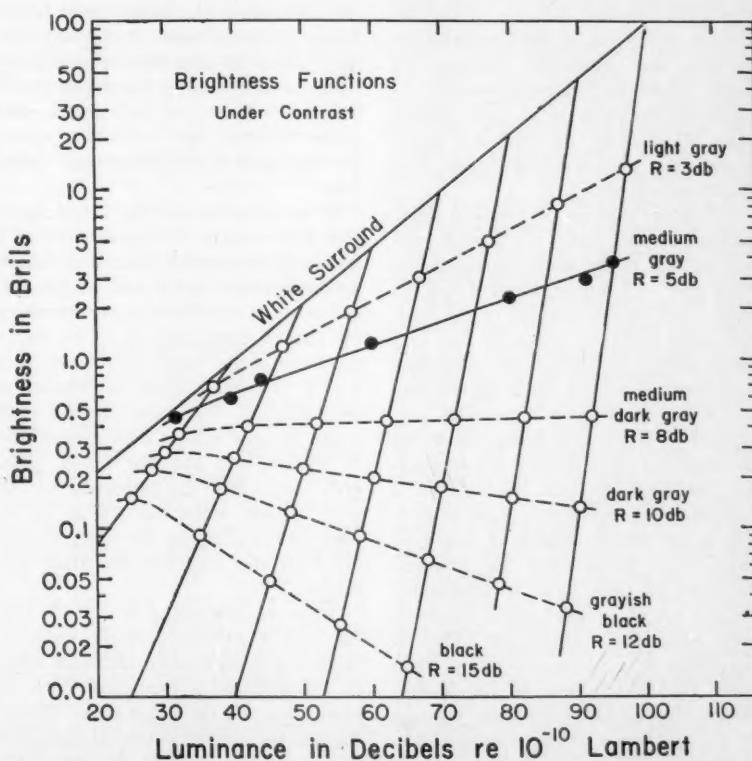


Fig. 4. A generalized set of functions showing the brightness of a target disk seen in the presence of a surround. The brightness function for the white surround is shown by the top line (slope = 0.33). Depending on the luminance of the surround, the brightness function for the target follows one or another of the steeper straight lines. The dashed curves pass through the points (circles) representing given fixed ratios, R , between the luminances of disk and surround. These ratios are stated in decibels. Thus, the top dashed curve is for a target disk 3 decibels below the surround in brightness. If the surround is a white paper, a darker paper 3 decibels down would be called light gray, as indicated. So-called brightness constancy is a manifestation of the relative flatness of the dashed curves. The filled circles represent data obtained by Leibowitz *et al.* (33), who matched a luminance seen in a black surround to a medium gray seen in a white surround under a wide range of illuminations.

vibration was one of the several procedures used to verify the mysterious fact that listening with two ears is different from listening with one. For some strange reason, the binaural exponent of the loudness function is about 10 percent larger than the monaural exponent (Table 1), and this small difference showed up when the two loudness functions were determined by comparisons with vibration applied to the tip of the finger, just as it manifested itself when the observers "matched" numbers to binaural and monaural loudnesses under the methods of magnitude estimation and magnitude production (30).

These procedures established the fact that for a sound pressure level of about 90 decibels the loudness of a sound as heard in two ears is precisely twice as great as the loudness heard when the same sound is delivered to only one ear. This 2-to-1 relation obtains only at one level, however, because of the difference in the size of the exponents for binaural and monaural listening. At lower levels the binaural-monaural ratio is smaller, whereas at levels greater than 90 decibels the binaural sound seems more than two times louder than the monaural sound.

3) In the domain of practical applications, an area that is not always without interest to the academic mind, the sone scale of loudness, the first and the most carefully documented of the modern ratio scales of sensation, has long since proved its utility to the acoustical engineer (the sone is the subjective unit of loudness). This scale recently performed its bit as an essential link in the development of a method for computing the total binaural loudness of a complex sound spectrum, given an analysis of the sound in terms of octave or third-octave bands (31). The loudness in sones of each band is determined from a set of equal-loudness contours, and the loudness values are added up according to a simple weighting function. To the loudness of the loudest octave band is added 0.3 times the sum of the loudness in the remaining bands. A version of this procedure is in fairly widespread use and is being readied as a secretariat proposal for general adoption by the International Standards Organization (32). The relevance of all this to our present concern is merely to show that ratio scales of sensation have their utility in the world of practical decisions.

4) The extension of brightness scaling to those circumstances under which a target is surrounded by a brighter background has led to some predictions that to me are rather startling. The full story is told elsewhere (9), but briefly it is this. We know that a bright surround depresses or inhibits the subjective brightness of a target in a most striking manner. This is the same inhibitory effect that we all experience when the glare of the oncoming headlights renders objects beside the road either dim or invisible. When a target luminance is subjected to the inhibitory contrast imposed by a brighter surround, the exponent governing the apparent brightness of the target jumps to a larger value. At high over-all levels the exponent grows by a factor as great as 10, but at lower levels the value of the exponent is smaller. These exponents are depicted by the slopes of the steep lines in Fig. 4, where we see that the steepness is less at the lower levels. These functions were determined in a long series of experiments aimed directly at determining the slopes of the brightness functions for targets seen under contrast. The brightness values are expressed in subjective units called brils.

If we want to use the functions in Fig. 4 to predict the appearance of a gray paper of a given reflectance viewed against a white background under various levels of illumination, we introduce the dashed lines, each of which shows the locus of the target luminances that bear a fixed ratio to the luminances of the surround. Put more simply, each shade of gray has its own dashed line in Fig. 4, and the line for a given gray shows how the brightness of the gray behaves when we change the illumination falling on the scene, including both the gray and its white surround.

What seems startling in Fig. 4 is that for some shades of gray the apparent brightness is supposed to decrease when the illumination is increased. Turn on more light and the target looks darker! That is the verdict of the dashed lines that slope down toward the right. This prediction has been checked with six observers who, having spent about 10 minutes in adapting to darkness, viewed a dark gray on a white background. The darkness of the target seen in dim light grew suddenly deeper when the illumination was suddenly increased by 10 or 20 decibels. The observers found

it especially interesting to watch the target turn gradually blacker as the illumination was gradually increased.

Many other interesting deductions can be made from the functions in Fig. 4. But what we have considered is enough to show that it is indeed possible to use the new psychophysical law and the procedures by which it was established in order, as James put it, "to test a theory or to reach a new result."

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Science in the News

The New Administration: A Report on Education; Taming the Rules Committee; Disarmament Activity

The report brought in by Senator Kennedy's task force on education called for an immediate increase in federal aid to education of \$2.5 billion for the next fiscal year. Most of the money would go for public schools; special attention would be paid to the poorer states, mostly in the South, where the quality of the schools is low even when a higher percentage of tax money is being spent on schools than in the richer states, and to cities of over 300,000, which not only face special problems but which have been habitually short-changed on their share of state funds by the rural-dominated legislatures that exist in most states. The task force, which was chaired by President Frederick L. Hovde of Purdue, asked for nearly \$1.5 billion a year, to be used primarily for classroom construction and raising teachers' salaries. The remaining billion dollars, for higher education, would be used mostly for construction and for an expanded student loan program.

Kennedy was noncommittal as to how much of this program he would ask Congress to enact. Few people think there is any realistic chance of his getting Congress to go along with all of it, although almost everyone agrees that, after 10 years of frustration, a substantial aid-to-education bill will be passed this year. The chances are that Kennedy will ask, at a minimum, for very prompt Congressional consideration of a school construction bill as a measure to alleviate the recession, which another task force warned is deepening.

The economic task force, chaired by Paul Samuelson of MIT, urged Kennedy in effect to push his full line of major spending programs. "Pledged expenditure programs that are desired for their own sake should be pushed hard. If 1961-62 had threatened to be years of over-full employment and ex-

cessive inflationary demand, caution might require going a little easy on them. The opposite is in prospect."

The task force recommended an increase over Eisenhower's proposals for the fiscal '62 budget of \$3 to \$5 billion, which, it said, "would not involve the inflationary risks of an all-out anti-recession blitzkreig." The task force's examples of "pledged programs desirable for their own sake" included school and college construction and more funds for medical research. Such recommendations, and those of other Kennedy task forces, show the sort of thing Kennedy has in mind, since their conclusions could have been predicted by anyone, including Kennedy of course, who was familiar with the views of the people chosen to study the problem. But Kennedy cannot possibly, during this session of Congress, ask for all the things his task forces have been recommending.

Program Undefined

The Kennedy legislative program at this point is in general almost completely undefined. The five-point program outlined a month ago, including unspecified aid to education, covered only items that were very nearly passed, and in one case, passed and vetoed, during the last Congress. It represented the absolute minimum for which Kennedy conceivably could ask. The rest of the program is not expected to be announced until after the inauguration, understandably, since the new Administration not only needs time to decide which of the many things it would like to do can be done during the present session, but also because presentation after or at the inauguration will not only have greater public impact but will give the opposition a little less time to get itself organized. For the present, Kennedy seems to be using the well-publicized, properly spaced reports of his task forces to create a feeling of urgency and activity without publicly committing himself to anything.

There were strong indications that a major block to any Kennedy legislative program, the House Rules Committee, which killed last year's school bill, would be effectively throttled before inauguration. Until Congress reconvened there was a good deal of talk that the six-to-six conservative versus moderate-to-liberal ratio of the committee would be broken by adding members to give the pro-Administration forces the majority needed to take action. It was supposed that this would be a more painless solution than the alternative of purging a conservative Democrat, and would be easier to put across than a rules change to deprive the committee of its power to block legislation.

This strategy, almost entirely in the hands of Speaker Rayburn, was changed, and there was no opening day effort either to add to the committee or change the rules. House liberals accepted, with noticeable apprehension, Rayburn's assurance that it would be better to purge Colmer of Mississippi and that Rayburn would see that this was done. His argument was that either the packing move or a rules change might conceivably be beaten by a Republican-conservative Democratic coalition, but that the Republicans would not dare oppose a purge of Colmer since this would mean breaking the long tradition that one party does not interfere with the other party's choice of its representatives on committees. Such a move would lay conservative Republicans open to unpleasant retaliation should the liberals increase their strength in the next election.

The formal justification for Colmer's purge would be that he opposed Kennedy's election, supporting instead that independent slate of electors which gave Mississippi's electoral votes to Senator Harry Byrd. The procedure would be for the Democrats on the Ways and Means Committee, who are responsible for choosing Democratic committee members, to replace Colmer with a more liberal Southern Democrat. Rayburn claimed to have a majority on the Ways and Means Committee to go along with this. It would then be confirmed by a caucus of House Democrats, where the liberals have a clear majority, and finally would be pushed through the full House. It was widely believed that a good many conservative Southerners who felt they had stuck their necks out to support Kennedy ac-

tively were not entirely averse to seeing Colmer humiliated.

Rayburn himself may have been motivated by something other than the tactical reasons he gave for this procedure. Two years ago he muted a liberal attack on the Rules Committee with assurances that he would use his influence with Colmer and Judge Smith, the conservative Democrats on the committee, to see that the committee did not block any legislation supported by a clear majority of the House. It became unmistakably clear during the last session that Rayburn lacked the power to keep this promise. It is a good deal less awkward for a politician to break an election pledge than a pledge made to his fellow politicians, and the old gentleman undoubtedly was planning to take some pleasure in reminding Mr. Colmer in the most forcible possible way that it does not pay to make Sam Rayburn look silly.

By last weekend there was not much doubt that Rayburn held the upper hand, for Judge Smith was distributing statements to the press announcing his readiness to accept "any honorable compromise," and specifically his readiness to agree not to block any of the five items on the announced Kennedy program, all of which the committee blocked or tried to block last session. Smith also hinted that he was willing to go along with a rules change to take away the committee's power to prevent a House-Senate conference to reach agreement on bills passed in differing forms by the two. (This power was used last summer to kill the aid-to-education bill.)

Early this week Smith went to Rayburn to offer his honorable compromise, but the Speaker offered, in return, only a choice between packing or purging the committee, which did not strike Smith as either honorable or a compromise.

Disarmament Appointment

The designation as head of the U.S. Disarmament Administration of John J. McCloy, former High Commissioner for Germany, former president of the World Bank, and lately chairman of the Ford Foundation, the Council on Foreign Relations, and the Chase Manhattan Bank, demonstrates the new Administration's serious interest in disarmament more forcefully than Kennedy's repeated calls, during and preceding the presidential campaign, for a more determined effort in this area.

Eisenhower, too, appointed a well-known figure to head the American disarmament effort when he chose Harold Stassen in 1955, but the situation was quite different. Stassen was a man prominent enough in Republican politics to have a claim to a position in the Eisenhower Administration, but no one was particularly upset when Dulles largely ignored what Stassen was trying to do. There were complaints that the Administration was ignoring disarmament, but not that it was ignoring Harold Stassen, although people who worked under Stassen, most of whom are now in the Disarmament Administration, feel that he did quite a good job and that we would probably be better off today if he had been listened to.

McCloy and Stassen

McCloy, though, is in a very different position from Stassen. He is a Republican, and although most of his government service was under Truman, he had no automatic claim to a job. His personal prestige is great enough so that it would be simply stupid politics to appoint him to an important-sounding post and then ignore him. No one has ever accused Kennedy of stupid politics. And the choice of McCloy to head the Disarmament Administration is only one of several Kennedy moves that reflect a determination to do, or at least try to do, something about disarmament, as opposed to merely talking about doing something. He has appointed Paul Nitze, chief of the State Department policy planning board during the last Democratic Administration and a man with a strong interest in the problem of disarmament, to be Assistant Secretary of Defense for international affairs, which include disarmament. Two of his closest advisers of long standing, Walt Rostow and Jerome Weisner, attended the recent Pugwash (scientist-to-scientist) conference in Moscow. Both men are assumed to be in line for important posts in the new Administration if they want them.

None of these developments is in any way surprising. It would be shocking if a man who had talked as much as Kennedy has of the need for a major increase in effort on the disarmament problem did not move quickly to get things going, particularly since one aspect of the problem, the Geneva test-ban talks, will be something the new Administration must deal with as soon as it takes office.

The outlook at the moment for a

test-ban agreement is not bad. The two major unresolved questions are the number of inspection stations to be set up within the Soviet Union and the number of inspections to be permitted. The two sides are close enough together on the number of stations to put a compromise within easy reach: the Russians are willing to accept 15; we want something over 20. On the number of inspections there is wider disagreement. We, again, want the right to make something over 20 per year; the Russians, on frankly political rather than scientific grounds, say they will accept only three and then with stricter limitations than we find acceptable. The possibility of compromise depends in large part on the Russian willingness to set aside a little farther their deep distaste for foreigners poking around in their country, and on the United States' willingness to accept a further increase in the already very substantial risk of evasion.

The problem is touchy for both sides, but there are grounds for restrained optimism. From the American point of view this is based on a certain amount of evidence that the Russians are beginning to take an increasingly serious interest in the problems relating to disarmament and the avoidance of war and that they may therefore gradually become more willing to relax their antipathy toward inspection, in turn the United States, encouraged by and to encourage this tendency in the Russians, may be willing to increase its risks by giving a little more on test-ban inspection now to lay a base for broader inspection agreements in the future.

Aside from some action on the test ban, no one expects any dramatic moves by the new Administration in its early months, partly because it will take some time for it to organize its policy; partly because the Administration, if it feels it can work out an acceptable test-ban agreement with the Russians, will probably want to overcome domestic opposition to that agreement before complicating the problem with other moves; and, most important, because the entire area of disarmament, stability, and arms control is so enormously difficult that it is unrealistic to expect, even assuming a genuine interest on both sides, that we and the Russians will be able to work out any major agreements overnight.

Nevertheless, with all the difficulties, in both the political and scientific communities there has been an accelerating increase, since about 1957, in the

amount of effort going into hard-headed, realistic studies of what can be done to diminish the chance of war aside from maximizing the power of the Western bloc as a deterrent. The effort has now been institutionalized at a very high level of the government in the Disarmament Administration, whose chief will hold a rank equivalent to Under Secretary of State.

Most Americans involved in this work feel that the Russians are being a good deal slower than we are in preparing to deal realistically with the problem, and there is a fairly sharp difference of opinion among the Americans who attended the latest Pugwash conference as to how far the Russians have come. The Russian delegates were well informed about the results of studies made by American students of the problem, but were slow to concede the validity of the studies, many of which point up the enormous difficulty of working out satisfactory agreements, even conceding a genuine desire to do so on all sides. The Russian reluctance to accept some of the less palatable American analyses is fairly understandable, considering the number of Americans who are quick to dismiss the difficulties thrown up by these analyses (such as, for example, those discussed in Fred Iklé's article in the current *Foreign Affairs*) as merely the work of people who don't believe in disarmament and who are interested only in trying to throw up roadblocks.

A basic point implicit in such analyses is the lack of any basis for an assumption that almost any disarmament agreement is better than none. Not only abstract analyses of what may happen in the future, but concrete analyses of the actual effects of such earlier disarmament efforts as the naval limitations treaty of the 1920's, show that what wishful thinking recommends as the road to peace, unpleasant realities may eventually demonstrate was a step toward war.

The restrained optimism that can be found among people working in this area today comes not from any expectation that great developments are on the horizon, but from the feeling that the mere fact that the two strongest powers are beginning to think more realistically and to talk more and more frankly about the steps that could lessen the chance of war is itself a step toward lessening the chance of war, and perhaps a more important step than any specific agreements that might result.—H.M.

News Notes

National and International Atomic Energy Groups Sign Cooperation Agreements

An agreement for cooperation between the International Atomic Energy Agency and the Inter-American Nuclear Energy Commission became effective on 22 December when it was signed in Washington by Sterling Cole, IAEA director general, and Jesse Perkinson, executive secretary of IANEC. A few days later another international atomic agreement was concluded when the European Atomic Energy Community, the United Kingdom Atomic Energy Authority, and the United States Atomic Energy Commission announced that they had decided to pool their efforts to collect and disseminate information concerning translations of literature in the field of nuclear physics, especially translations from languages unfamiliar to Western readers, such as Russian and Japanese.

IAEA-Latin American Terms

The first document signed was a relationship agreement that had previously received unanimous approval from the IAEA General Conference and from the Council of the Organization of American States. Under its terms, IAEA and IANEC "will act in close cooperation with each other and will consult each other regularly in regard to matters of common interest." Among the types of cooperation envisaged in the agreement are exchange of information and documents; close working relationships between the staffs of the two organizations; arrangements for the cooperative use of personnel, materials, services, equipment and facilities; and reciprocal representation at meetings.

The commission has already rendered valuable assistance to IAEA in its activities in Latin America—activities such as holding training courses and organizing assistance missions. The IAEA, for its part, has previously recognized the connection between the work of the two organizations by inviting IANEC to send observers to the IAEA General Conference.

The agreement with IANEC is the second of its kind to be concluded by IAEA with a regional organization. An agreement with the European Nuclear Energy Agency became effective in November 1960.

The Translation Agreement

Under the Euratom-UKAEA-USAEC agreement, a central information office, Transatom, has been established at Euratom's Brussels headquarters.

It will function in two ways. First, it is publishing a monthly *Transatom Bulletin* (the first issue was December 1960) which will list existing translations recently reported to the Brussels office, as well as new translations planned by international or national institutions and private firms in the European Community, the United States, the United Kingdom, and other areas.

Second, all data relating to translations, including translations made before the establishment of Transatom, are being collected and recorded in a master file in Brussels. Copies of this card file have been offered to appropriate institutions in countries with great interest in the nuclear field.

Efforts are being made to avoid duplication of work when the European Translation Centre, to be established at Delft (Holland), is set up. The scope of that institution is much wider: it will cover all scientific and technical material in the field of exact sciences.

The *Bulletin* is available on a subscription basis from: Transatom, c/o Euratom, 51 rue Belliard, Brussels, Belgium, at \$8 a year, air mail \$16.

Zoologists Speak Out on Birth Control and on Animal Use Law

Zoologists pioneered in taking a public stand on two major controversial issues when a resolution urging government support of birth control research and training and another opposing a Senate bill that would regulate the use of laboratory animals were passed by the American Society of Zoologists at its annual meeting in New York on 29 December. The birth control resolution says:

"The American Society of Zoologists views the mounting rate of population growth, especially in the world's poorest areas, as a principal factor contributing to global conditions of human misery, famine and under-education, and we urge our Government to adopt policies in keeping with this country's tradition of deep sympathy for human suffering.

"In many countries, officially adopted policies of voluntary fertility control are rendered ineffectual, and virtually in

all countries, including our own, the desire of some segments of the populace for smaller families remains unfulfilled because of the lack of simple, acceptable methods for the voluntary regulation of human reproduction. The freedom to limit family size is still a privilege of the educated few and should be extended to all people. Furthermore, the distribution of scientific and medical information should be unencumbered by restrictive laws or prejudicial attitudes.

"As biological scientists, we recommend that our Government implement a policy indicative of its dedication to the service of mankind by assuring the full support of its appropriate agencies to research in the biology of reproduction and fertility control and by offering programs of assistance in these fields for the training of American as well as foreign scientists."

The resolution on proposed legislation on the use of laboratory animals reads:

"Senate bill 3570 would regulate the use of all vertebrate animals in research and teaching. Such legislation would reduce freedom in day-to-day planning and conduct of research and would reduce the amount of research in many areas which provide basic information for medicine, agriculture and conservation of animal resources. This proposed legislation is discriminatory in its implication that animal biologists are suspect and inhumane in laboratory practice. The American Society of Zoologists, assembled in convention in New York City in December, 1960, strongly opposes Senate bill 3570."

AAAS Laurentian Hormone Conference Announced

The 1961 Laurentian Hormone Conference of the AAAS will be held at Hoburg's Resort, Lake County, Calif., 4-9 September. Participants from abroad will include Jamshed R. Tata, Wenner-Grens Institute, Stockholm, Sweden; J. B. Brown and G. D. Matthew, Clinical Endocrinology Research Unit, Edinburgh, Scotland; and C. W. Emmens, Department of Veterinary Physiology, Sydney, Australia.

Investigators interested in attending should make application to the Committee on Arrangement of the Laurentian Hormone Conference, 222 Maple Ave., Shrewsbury, Mass., at an early date and in any event *no later than 10 May*. A conference rate of \$14.50 per

day per person is extended to all invited participants, with reduced rates for children. Since the number of participants is necessarily limited by available accommodations, all applications are screened and invitations to attend are issued by the second week in June. Invitations issued to applicants will include members of their families, and indication of the number of such members should be made upon application to the committee.

Solar Telescope Construction Begins

At Kitt Peak National Observatory, Tucson, Ariz., construction of the world's largest solar telescope is under way. The instrument is being built for the National Science Foundation by the Association of Universities for Research in Astronomy, Inc., operator of the new national observatory. The NSF has allotted \$4 million to the Kitt Peak solar program.

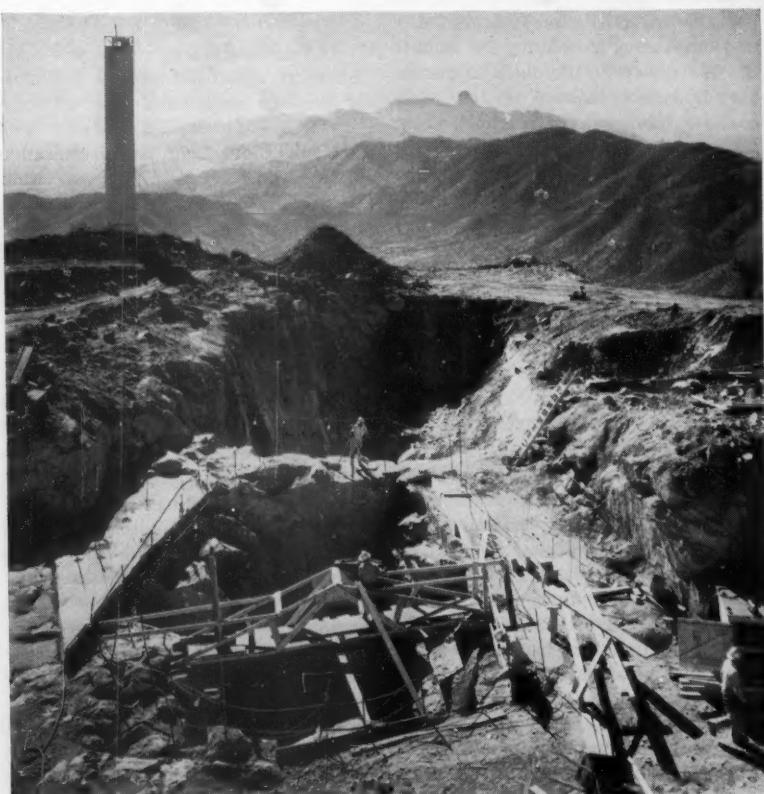
The distinguishing feature of the

construction is a hole 380 feet long, cored into the top of Kitt Peak, a mountain 40 miles southwest of Tucson on the Papago Indian Reservation. A deep tunnel, 15 feet in diameter and blasted into the mountain at an angle of approximately 32 degrees, it looks more like a mine shaft than an astronomical research facility.

Rising nearby is a large pillar of steel and concrete upon which will rest a motor-driven flat mirror, called a heliostat. Standing 110 feet above the ground, the heliostat will track the sun and reflect an image of it down an angled shaft extending from the top of the pillar to the bottom of the mountain tunnel, a total distance of almost 500 feet.

From that point the solar image will be reflected 280 feet to a third mirror, which will project it into an underground observing room. There the image—as large as 34 inches in diameter—may be photographed or directed to spectrometers for study by scientists.

As Alan T. Waterman, NSF direc-



Site of the solar telescope at Kitt Peak National Observatory in the Quinlan Mountains of Arizona. The large hole in the foreground will contain the observing room. The tower at left contains meteorological instruments used in the site survey. Famous Baboquivari Peak in the distant background is the sacred mountain of the Papago Indians, on whose reservation the national observatory is located.

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tor, pointed out recently, "when completed, the huge instrument will give solar researchers more revealing views of the sun than have ever been possible from the earth. Such observations will increase substantially man's meager knowledge of the star that keeps our planet alive." A. Keith Pierce, associate director of the Kitt Peak Observatory, is in charge of the solar program. He expects the solar instrument to be completed in 1962.

The solar telescope is one of several major instruments in use or under construction at the new national research center for optical astronomy. A 36-inch stellar reflector has served researchers since the observatory was dedicated in March. The housing for an 80-inch stellar reflector is nearing completion. Grinding of the mirror for the reflector will begin next year, according to Nicholas U. Mayall, observatory director.

Major Temperature Symposium Scheduled

The technical program is now complete for the most comprehensive meeting ever to be held on the subject of temperature measurement, the 1961 Symposium on Temperature, Its Measurement and Control in Science and Industry, to be held in Columbus, Ohio, 27-31 March. About 200 papers will cover temperature measurements from absolute zero to 10,000,000°K and will deal with subjects ranging from the temperature of the human body to the temperature of the sun. Almost every area of the physical, biological, medical, and engineering sciences will be represented. The conference is sponsored by the American Institute of Physics, the Instrument Society of America, and the National Bureau of Standards.

Temperature ranks as one of the most important of the physical quantities, and efforts to measure and understand it have provided some of the most difficult and challenging problems in experimental and theoretical physics. Accurate measurement of temperature is becoming increasingly important in the automatic control of industrial processes, in engineering applications of atomic energy, and in the development of high-speed aircraft and missiles.

Symposium participants who will present the 200 papers in March come from universities, government and

military research laboratories, and industrial research laboratories in this country, as well as from research centers in Australia, Germany, Netherlands, Canada, Soviet Russia, and Great Britain.

Three international symposia have previously been held to permit exchange of information among the various groups that work with extreme temperatures and precision temperature measurement and to stimulate pertinent research. The first symposium was held in Chicago in 1919; the second, in New York City in 1939, the third meeting, in 1954, dealt with fundamental concepts and summarized the developments that had taken place since 1939. The proceedings of these symposia, *Temperature, Its Measurement and Control in Science and Industry* (Reinhold, New York), have become basic reference works in the field.

Persons interested in the March symposium may obtain further information by writing to V. W. Sikora, Instrument Society of America, 313 6th Ave., Pittsburgh 22, Pa.

News Briefs

Defense chief to stay. Herbert F. York, Defense Department director of research and engineering, has agreed to stay on for a "limited period" after the Administration changes hands on 20 January.

Explorer VIII silent. Explorer VIII, the ionospheric direct-measurement satellite, is no longer transmitting. The last signal heard from the 90-pound craft, which was launched by the National Aeronautics and Space Administration on 3 November from Cape Canaveral, was received at Quito, Ecuador, on 27 December. A useful lifetime of between 2 and 3 months had been predicted for the satellite, which was entirely dependent for power upon a self-contained battery pack. More than 700 miles of magnetic tape have been used to record the data transmitted.

Ocean floor to be drilled. In March, test drillings are scheduled to begin in the project to pierce the earth's crust and reach the mantle. The site chosen is 40 miles east of Guadalupe, an island off the west coast of Mexico, in 12,000 feet of water. The depth of the earth's crust under the oceans is only a few

miles, whereas under the continents it is many times deeper.

During the month of test operations it is hoped that three or four 1200- to 1500-foot holes can be drilled in order to gain experience and information that can be used in designing a rig to bore 16,000 feet, the depth necessary to accomplish the final mission.

The National Academy of Sciences and the National Science Foundation have announced that a contract has been awarded to the Global Marine Exploration Company of Los Angeles for the test work off Mexico. Willard Bascom is technical director of the project, known as Mohole.

Egyptian expedition. An expedition to Egypt, sponsored jointly by Yale and the University of Pennsylvania Museum, leaves this month as part of an international project to rescue ancient ruins which otherwise will be flooded forever in the next 4 years by the backwaters of the new Aswan Dam. The three-year project will be financed by a \$45,000 grant from the Bollingen Foundation and a matching fund from the Eckley B. Coxe, Jr. Fund of the University of Pennsylvania.

William K. Simpson, of Yale's Peabody Museum, will be in charge of the expedition. Plans have been made to establish a Yale-Pennsylvania camp in Nubia, 20 miles north of the Second Cataract on the eastern bank of the Nile and under the shadow of the temple of Abu Simbel, built by Rameses II in 1270 B.C. The team of archeologists will spend the first 3 months of 1961 in Egypt; two other trips are scheduled for 1962 and 1963.

Mercury administrative shift. The National Aeronautics and Space Administration has announced that its Space Task Group, charged with carrying out Project Mercury and other NASA manned space flight programs, has officially become a separate NASA field element. Located at Langley Field, Va., Space Task Group, which is composed of some 600 persons, has been reporting administratively to the Goddard Space Flight Center at Greenbelt, Md. Under the new organization, group director Robert R. Gilruth will work directly under Abe Silverstein, NASA director of Space Flight Programs, Washington, D.C.

Chemistry meeting. The 18th International Congress of Pure and Applied Chemistry, to be held in Montreal,

Canada, 6-12 August, invites submission of contributed papers for all sections of the congress except the wood and organic chemistry symposia, which will have invited papers only. English, French, or German abstracts of not more than 200 words must be submitted by 15 February to the Secretary of the Central Committee, 18th International Congress of Pure and Applied Chemistry, National Research Council, Ottawa, Canada.

* * *

Physics apparatus competition. The Committee on Apparatus for Educational Institutions of the American Association of Physics Teachers has announced that, as part of its program to make new and improved apparatus available to physics teachers, it will conduct a competition for new apparatus for the teaching of college physics. Prizes will be awarded for new pieces of apparatus which are judged most likely to advance the teaching of college physics. New apparatus includes that which is neither commercially available nor well known through published descriptions.

Apparatus entered in the competition will be exhibited, and the judging will take place, at the Annual Meeting of the American Association of Physics Teachers, 1-4 February, at the Hotel New Yorker, New York City. The competition is supported by a grant from the Central Scientific Company.

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Free radicals. The Fifth International Symposium on Free Radicals will be held at the University of Uppsala, Uppsala, Sweden, 6-7 July, under the auspices of the university's Institute of Physical Chemistry. Each session will start with a plenary lecture, followed by 10-minute contributed papers. Preprints will be available at the opening of the symposium. Titles must be submitted by 31 January, and the manuscripts of the papers accepted must be submitted by 1 May. All correspondence should be addressed to: Fifth International Symposium on Free Radicals, Institute of Physical Chemistry, Uppsala, Sweden.

Scientists in the News

George W. Beadle, Nobel Prize-winning geneticist of California Institute of Technology, has been named chancellor of the University of Chicago. He will relinquish his responsibilities

as acting dean of the institute's faculty and chairman of the Division of Biology within 90 days, to assume his new post. He succeeds Lawrence A. Kimpton, who resigned last September to join the Standard Oil Company (Indiana).

Beadle, a past president of the AAAS, is a member of the President's Science Advisory Committee and of the Committee on Genetic Effects of Atomic Radiation of the National Academy of Sciences-National Research Council, as well as chairman of the Research Advisory Council of the American Cancer Society.

Wallace Brode, former State Department science adviser and a past president of the AAAS, was elected president of the Society of the Sigma Xi at the national meeting of the society that took place on 29 December during the AAAS meeting in New York. Brode, who succeeds **Frank M. Carpenter** of Harvard University, will serve until 1963. **Frederick D. Rossini** of the University of Notre Dame was named president-elect.

The unusual simultaneous election of both a president and president-elect was occasioned by the withdrawal from the presidency of **Donald B. Prentice** of Yale University. Prentice, voted president-elect 2 years ago, was advised by his physician that he should not serve and asked to be relieved.

Two new members of the Sigma Xi executive committee were also named at the New York meeting—**Harry L. Bowman** of Drexel Institute of Technology and **E. F. Osborne** of Pennsylvania State University. They replace retiring members **Detlev W. Bronk** of the Rockefeller Institute and **W. T. Ziegler** of Georgia Institute of Technology.

The Franklin Institute has announced the retirement of **Nicol H. Smith** as director of the institute's Laboratories for Research and Development. Until a successor is named, **J. G. Richard Heckscher**, executive vice president of the Franklin Institute, will direct the laboratories.

Smith has been laboratories director for 11 years. He joined the institute staff in 1932 as associate director in charge of chemistry for the science museum. When the laboratories were formed in 1946, he was appointed director of chemical engineering and physics. Three years later he became the over-all director.

Smith, who holds a bachelor's, master's, and doctor's degree from the University of Pennsylvania, is recognized for his technical work on the atomic weight of scandium and for his research on germanium, tungsten, and magnesium oxychloride cements.

George M. Murphy, professor and head of the department of chemistry at New York University, has been named to the newly created post of associate dean of arts and sciences at N.Y.U. He will have special responsibility for instructional and research programs in science.

While a research assistant at Columbia University in the 1930's, Murphy, along with Harold C. Urey and F. G. Brickwedde, shared in the discovery of deuterium. Urey received the 1934 Nobel prize in chemistry for his work in the project.

Victor A. Sutter of El Salvador, his country's Minister of Health and Welfare from 1956 to 1958, has been appointed secretary-general of the Pan American Sanitary Bureau, Washington, D.C., regional office of the World Health Organization. Sutter has a long background in the field of international health; he spent 6 years at WHO headquarters in Geneva, first as director of the Divisions of Communicable Disease and Public Health Services and later as assistant director-general in charge of Advisory Services.

The Soil and Health Foundation in Allentown, Pa., has awarded a \$4000 Dental Research Prize to **Albert Schatz**, chief of the division of microbiology at the Philadelphia General Hospital and associate in medicine at the University of Pennsylvania Graduate School of Medicine. Schatz is internationally recognized as the codiscoverer of streptomycin and is well known for his proteolysis-chelation theory of dental caries, a new approach, derived in part from the work of Charles F. Bodecker, to the problem of tooth decay.

Harold S. Osborne of Upper Montclair, N.J., retired chief engineer of the American Telephone and Telegraph Company, has won the Edison Medal of the American Institute of Electrical Engineers. The award will be presented at the institute's winter general meeting in New York, 29 January-3 February, at the Hotel Statler. Osborne, who is a

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Harold S. Osborne [Fabian Bachrach]

consultant with the International Electrotechnical Committee, was cited "for his contributions to the art of telecommunication and his leadership and vision in extending its application; for his achievements in the coordination of international communication and in national and international standardization; and for his advancement of the engineering profession."

John W. Bartlett will retire on 1 July as chairman of the dairy science department at Rutgers University. He will be succeeded by **John P. Mixner**, Rutgers professor, who has been on the staff of the Rutgers Dairy Research Farm in Beemerville since 1947. He is noted for his research in artificial breeding and the physiology of mammary-gland growth.

Alfred M. Mayo of the National Aeronautics and Space Administration has been elected president of the American Astronautical Society for 1961. Mayo joined NASA last May as assistant director for bio-engineering in the Office of Life Sciences, after 20 years with the Douglas Aircraft Company.

J. Desmond Clark, English anthropologist and archeologist specializing in prehistoric Africa, has been appointed professor of anthropology at the University of California, Berkeley, effective 1 July. Clark is director of the Rhodes-Livingstone Museum in Northern Rhodesia, where, since 1938, he has conducted field research in ethnology, paleontology, and prehistoric archaeology.

Floyd W. Denny, Jr., a member of the faculty of the University of North Carolina School of Medicine since 1952, has been named head of the department of pediatrics. He succeeds E. C. Curnen, Jr., who recently accepted a similar position with Columbia University's College of Physicians and Surgeons, New York.

Frederick Urbach, assistant medical director of the Skin and Cancer Hospital of Philadelphia, a unit of the Temple University Medical Center, has been appointed professor of research dermatology at the center.

John D. Spikes has returned to his regular position as professor and head of the department of experimental biology at the University of Utah after serving for 2 years as a specialist in cell physiology with the U.S. Atomic Energy Commission's Division of Biology and Medicine, Germantown, Md.

Recent Deaths

Douglas H. Fryer, Rye, N.Y.; 69; industrial psychologist and a retired professor at New York University; one of the first to apply psychological research methods to management and military problems; in 1930 became the first president of the Association of Consulting Psychologists, which established one of the first psychology clinics in New York; president of the American Association for Applied Psychology in 1937 and a vice president of the AAAS, in 1950, and chairman of its psychology section; had served as editor of various journals of psychology; 24 Dec.

Frank R. Ober, Boston, Mass.; 79; orthopedic surgeon known for his treatment of the effects of poliomyelitis; joined the Children's Hospital in 1913, and since 1946 had been chief orthopedic surgeon emeritus; from 1937 to 1946 was John B. and Buckminster Brown clinical professor of orthopedic surgery at the Harvard Medical School, with which he became associated in 1915; 26 Dec.

Maurice C. Pincoffs, Baltimore, Md.; 74; professor of medicine at the University of Maryland School of Medicine, where he had been on the faculty for 36 years; a brilliant diagnostician, served as physician-in-chief at University Hospital from 1922 to 1954, simul-

taneously heading the medical school's department of medicine; from 1954 until retirement in 1957, organized and directed a new department of preventive medicine and rehabilitation; 8 Dec.

Herbert F. Prytherch, Salisbury, N.C.; 62; former director, U.S. Fish and Wildlife Service Laboratory, Beaufort, N.C., from 1931 to 1947; well known for his studies on the biology of the oyster; 26 Dec.

Burch Rachlis, Philadelphia, Pa.; 64; ear, nose, and throat specialist and a member of the Temple University medical faculty; 31 Dec.

Frederick E. Russell, Louisville, Ky.; 90; public health specialist who developed the first successful typhoid vaccine while serving in the Army, from which he retired in 1920 as a brigadier general; former general director of the International Health Board of the Rockefeller Foundation and former professor of preventive medicine and epidemiology at Harvard Medical School; 29 Dec.

Erwin Schrödinger, Vienna, Austria; 73; eminent mathematical physicist and co-winner, with P. A. M. Dirac, of the 1933 Nobel prize for developing the wave mechanics theory to explain seeming contradictions in the nature of light; taught at the universities of Vienna, Stuttgart, Breslau, and Zurich; in 1927 succeeded Max Planck as head of the department of technical physics at the University of Berlin; with the rise of Hitlerism, went to Oxford, Graz, and the Dublin Institute for Advanced Studies, where he stayed from 1938 to 1956, when he returned to the University of Vienna; noted for contributions to the theory of matter, the physical theory of colors, and the quantum theory; in Dublin, set forth a new theory for the origin of life, and in 1947 reported that he had succeeded in expanding Einstein's theory of relativity and in proving the possibility of utilizing it for research in the field of electromechanical energy; 4 Jan.

E. J. Tiffany, Atlanta, Ga.; 59; former Public Health Service officer who until his retirement last year was chief training officer for the laboratory branch of the Communicable Disease Center in Atlanta; former associate professor of bacteriology at the Long Island College of Medicine; 25 Dec.

Erratum: On page xix in the index to volume 132 [Science (30 Dec. 1960)], the entry in line 32, column 2 (Powers, E. L. See C. F. Ehret, 1768) should appear before the previous entry. E. L. Powers, rather than C. F. Powell, was co-author of the report "Reduction of radiation sensitivity of dry bacterial spores with hydrogen sulfide" on page 959.

Book Reviews

Evolution after Darwin. vol. 3, *Issues in Evolution*. Sol Tax and Charles Callender, Eds. University of Chicago Press, Chicago, Ill., 1960. viii + 310 pp. Illus. \$7.50.

In November 1959 nearly 50 distinguished scientists were gathered at the University of Chicago to discuss the impact of evolutionary thought on every field of endeavor, from astronomy to the control of our own destinies. The meeting was planned to celebrate the centenary of the publication of Darwin's *Origin of Species*. As a preliminary to their discussion, the specialists prepared and submitted 42 papers (later published as volumes 1 and 2 of *Evolution after Darwin*) bearing on the subject of evolution. The papers, circulated in advance to participants, were referred to a committee of 80 from the University of Chicago and were utilized by the committee in outlining a summary. During the celebration, the points of the summary were the basis for discussion by five panels: "The Origin of Life," "The Evolution of Life," "Man as an Organism," "The Evolution of Mind," and "Social and Cultural Evolution." Finally, 40 of the specialists were assigned to the panels. Most of the original papers were quite broad reviews of particular fields, and the contributors were apparently chosen to complement one another.

The operation, in keeping with the times, was complete with scientific "breakthroughs." (The indexer did not share this space age enthusiasm, so for "Breakthrough, See Transformation.") One can only admire the effectiveness with which the records of the Darwin celebration answer Sol Tax's question: "After one hundred years of Darwinian theory, where do we stand?" The third volume of *Evolution after Darwin* provides a simplified answer in the committee's outline of the major evolutionary issues, or conclusions (based on papers found in the earlier volumes),

and in the panelists' explanation of the outline. I use *explanation* because the panelists apparently found the majority of the discussion topics to be agreeable generalizations that might need illustration or elaboration; the topics very rarely provoked discussion.

Transcripts of discussions are easily criticized, and the most stimulating discussion rarely shines under a reader's quiet scrutiny. The five panels seriously took up the task of surveying the full spectrum of evolutionary thought and, consequently, had little time for argument. Two of the panels provided some discussion by bringing together diverse groups of individuals badly in need of intellectual intercourse. Biologists, psychiatrists, and anthropologists discussed the evolution of mind, and anthropologists and biologists considered social and cultural evolution. Here there was no common ground of orthodoxy, and the discussions ranged widely to consider unsolved problems and to compare the research methods used by different workers as well as the analyses of their research.

The discussion of the origin of life was the most disappointing of all. R. W. Gerard, alone, went into the flights of romantic imagination that can reveal illuminating generalizations. Despite Gerard, the problem was presented as one in which progress is slow and difficult but, nevertheless, as an area in which there is wide agreement.

The one divergent opinion came from H. J. Muller who insisted that life should be defined as things with the genetic properties of deoxyribonucleic acid and that all else is lifeless. Many problems can be solved with this definition:

Muller: "My answer is that those who define life as I do will admit that the most primitive forms of things that deserve to be called living have already been made in the test tube by A. Kornberg."

With the origin of life explained,

the second panel took up the evolution of life. Any biologist unfamiliar with current views on natural selection will find this an easily read, highly informative discussion. Regrettably some evolutionists (mostly systematists, I suspect) are not familiar with current views; it is a measure of their need that Ernst Mayr found it necessary to say that pleiotropy is "taken for granted by geneticists but not fully understood by some evolutionists."

The discussion focused very largely on the ways in which genetic modification of stocks appears; the panel consisted of the men who have contributed most extensively to the consolidation of our present ideas on speciation. No other group could have summarized our present-day-stand as well, or with such authority.

Part of our knowledge of mechanisms has come from the comparative study of organisms, and the purely descriptive or historical approach to particular phylogenies is exemplified in the third panel's consideration of man as an organism.

The rarity of hominid fossils, and the uncertainty about dating these fossils, leaves us with a sketchy knowledge of the details of human evolution and supports considerable debate over interpretations. Very little attention was paid to the disputes, for the discussion revolved around the kinds of data that can be derived from fossils. L. S. B. Leakey demonstrated the precision and care that must be exercised in the interpretation of comparative anatomy. In order to cover the ground, the discussion had to move swiftly; a complex descriptive problem such as this cannot be effectively outlined by a panel.

At about the mid-point, the panels had completed the survey of the most active and productive areas of evolutionary thought. Generalizations about natural selection and an example of a phylogeny had been discussed. Leslie White, in his discussion on the last panel, clearly distinguished the two approaches. The "particularizing process, in which events are considered in terms of their uniqueness" is, of course, the study of histories or phylogenies and is quite different from the "generalizing process which deals with phenomena as classes" and which allows one to propose explanatory principles and processes. A phylogeny, when well enough established by sound data, could be called a fact, but no matter how val-

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able and unique an explanatory principle is, it will never be anything other than a sound model.

Anyone who sees a point to this distinction will be disturbed to find the second panel asserting "Biologists . . . take the *fact* of evolution for granted, as a necessary basis for interpreting the phenomena of life." It would be sounder and more truthful to say that natural selection provides the only available rational explanation for the phenomena of life and convinces us that evolution did occur. In view of our present understanding and knowledge of evolution, I feel that challenging religions and the state of Tennessee with the "fact of evolution" puts one at the level of an irrational minority that does not merit the notice of any of the Darwin centennial celebrants.

Evolutionary Vision

Sir Julian Huxley's introductory address anticipated problems that came up during the last half of the panel discussions. To Sir Julian "evolutionary vision" reveals an "evolutionary landscape" with "alarming monsters in our evolutionary path." Such monstrous problems as superscientific war, overpopulation, the rise and appeal of communist ideology, and the preoccupation with means rather than ends are, I think, supposed to be resolvable with evolutionary vision, and, I believe, the clue to it all was given in the fourth panel when Sir Julian said that "during the course of evolution on this planet quality has somehow arisen out of quantity and the subjective has arisen out of the objective." My evolutionary myopia doubtless does an injustice to such ideas.

The particulars behind the biologists' concern revolved around the disastrous consequences of current population trends and breeding systems. (Muller: "everyone is helped to live according to his need and to reproduce according to his greed.") Any desirable control must mean a change, and a very prompt change at that, in modern culture, but this will have to be a conscious, directed alteration of culture. The biologists seemed to be convinced that genuine knowledge of the nature and principles of cultural evolution could, perhaps, permit a wise and humane control to be exercised over our evolution.

Toward the end of the third panel's discussion, C. H. Waddington outlined

a most fascinating view of a sociogenetic system by asking how and why information obtains value and is transferred in society. An answer to this question was not suggested, but the question must be answered before the details of cultural evolution can be compared with organic evolution.

The mere analysis of behavioral elements is a young science in which the investigator's use of words still beclouds some issues. This is best indicated by a long quotation from the transcript of the fourth panel:

Huxley: But from my behavior you would deduce that I see a difference in color between this carpet and your clothes?

Tinbergen: Right there we are in the middle of semantic difficulties.

Huxley: That is to evade what is to me an obvious fact.

Tinbergen: It is an obvious fact to you, the subject who sees it; it is not an obvious fact to me. The obvious fact to me is that you react differently to the two colors and that you tell me so, which is part of your reaction.

Huxley: I must say I disagree with you. I think we have to believe that animals do perceive some difference of quality in colors, for instance.

Gerard: But do you call this a belief, or do you call it a fact?

Huxley: I think we have to believe that it is a fact, as we have to do with many other scientific conclusions.

It would be an error to consider this to be typical of the discussion or of the panel; I use it only to show the extremes of the philosophical approaches to behavioral science. Both men helped to present a very well-rounded summary of the discoveries of ethologists and aided the panel in outlining our present knowledge of complex mental activity. That the view is not yet evolutionary is a reflection on the problems facing behaviorists, not on their achievements. The panel provided a very brief but interesting consideration of comparative behavior and an especially provocative glimpse of the rare cases of transfer of heuristically developed behavior in animals.

On the last panel, an array of biologists and two cultural anthropologists (Julian Steward and Leslie White) entered into some pretty heated discussion with representatives of the main stream of cultural anthropologists. The point being considered was whether it is valid to generalize about principles

underlying cultural change. Even though particularization was distinguished from generalization, there seemed to be little appreciation of the fact that these two valid and necessary approaches are, in the end, complementary. This account is useful as a record of a face-to-face debate between very different schools of thought, but as evidence of the dissipation of energy through misunderstanding, it is discouraging.

It is always easy to criticize the lack of vision and progress outside one's own field, and the biologists, agonized over our population growth and genetic mismanagement, find it even more difficult not to criticize social scientists for their inability to suggest devices for cultural management. Unfortunately Julian Steward's remarks, which were directly concerned with causation, are inserts that were not delivered to and debated by the panel, for they show the views of an anthropologist who shares the prejudices of biologists.

In the ancillary remarks, Sol Tax gives the impression that the purpose of the celebration was to bring cultural anthropologists and biologists together in the hope of stimulating more interchange of ideas and attitudes. Perhaps biologists were too quick to attach significance to the similarities of biological and cultural evolution, and at times they were certainly naively enthusiastic about scientific, cultural midwifery as an alternative to political chicanery. This does not alter the fact that the search for generalizations and principles in the mass of anthropological data would appear to merit wider consideration; the Darwin celebration made strenuous efforts to bring this about.

Roughly a third of the volume is taken up with tag ends. Evolutionary views of modern theologians, early Chinese, and up-to-date Roman Catholics are essayed. The introductory address, concluding remarks, and transcripts of two television programs; some pictures that only an advertisement could call a photographic essay; an index to all three volumes; and Sol Tax's personal reminiscences of the disorder behind the order of the celebration are all included. Now those who did not attend can sit back with a full account of the affair and no worries as to which queue correlates with a round punch, a triangular punch, or no punch in their ticket; those who respond to meetings as I do will prefer the book.

By itself the third volume of *Evolu-*

tion after Darwin is anomalous and will not serve the needs of either the specialist or the general reader. The first two volumes, on the other hand, can stand alone as technical essays, but something quite intangible and very useful is added to their content when the panel discussions are read as an introduction to or a survey of the field. The very attenuated comments of the panelists focus ideas that are easily missed in the mass of detail in the technical papers, and the panel's generalities are shown to be derived from a much more diverse set of ideas than is implied.

Summary of the Celebration

When all three volumes are taken together, one's perspective is improved. There is still only brief consideration of the origin of life, but the evolution of life, so neatly and precisely sketched by the panel, is shown to be an immensely exciting, growing area of research. The bare outline of man as an organism becomes recognizable, in the second volume, as descriptive and comparative functional biology of the highest order. The diverse approaches used in considering the evolution of the mind were quite clearly covered in the papers, but the panel made it clear that the approaches must sometime converge. Finally, the logical essays of the second volume make the disunity of the panel on social and cultural evolution appear to be the result of an almost ritualistic defense against early Darwinian excesses, which is now giving way in the face of more sophisticated attempts at generalization.

The foolhardy attempt to summarize 1002 pages of technical papers with 174 pages of panel transcripts resulted in a good survey that can be usefully employed in conjunction with the first two volumes. And there was still time to consider the human implications of evolution which are of such great concern to us all.

It is a measure of considerable enthusiasm when I must agree with dust-jacket prose and say *Evolution after Darwin* is, in fact, "the most comprehensive and intensive examination ever made of the impact of Darwin's ideas." The three volumes do just honor to the occasion of the Darwin Centennial Celebration and to the thinking that Charles Darwin set in train.

RODGER MITCHELL

Department of Biology,
University of Florida

Atlas of European Birds. K. H. Voous.
Nelson, New York, 1960. 284 pp.
Illus. \$15.

The title of this book is literally correct in the old sense of the word *atlas*; it is a volume of maps, one for each of the 419 species of European birds breeding west of the Ural Mountains. Each map is accompanied by a closely written statement that includes the ancestral or inferred faunal placement of the species: for example, the black-winged kite, *Elanus caeruleus*, is given as "in Europe an Ethiopian distribution element," and the white-tailed eagle, *Haliaeetus albicilla*, is given as "paleoarctic"; this statement includes the geographic range, habitat, chief food, nesting habitat, and movements (including migration). Voous recognizes 24 faunal types—the arctic, holarctic, Siberian-Canadian, Siberian, Chinese-Manchurian, Palearctic, Nearctic, North Atlantic, European, European-Turkestanian, Turkestanian-Mediterranean, Mediterranean, Sarmatic (belonging to the coastal fauna that, in late Tertiary and Pleistocene time, inhabited the shallow, brackish, or salt Sarmatic inland sea, a continuation of the eastern Mediterranean stretching over the present Hungarian Plain, east to the Caspian and Aral Seas), Turkestanian, Paleo-xeromontane, Paleomontane, Tibetan, Mongolian-Tibetan, Ethiopian, Indian-African, Of the Old World, Antarctic, and Cosmopolitan. The present placement of five species is given as "unknown," since they provide no indication of the geographical origin of the five: the Manx and the North Atlantic shearwater, the Gannet, the Greater Flamingo, and the Black-winged Kite.

The maps, which are pseudo-Mercator projections, have the breeding range of each species marked in red. Most of the maps extend from the equator to the North Pole, but some—for the Caspian tern, the roseate tern, and others—extend to the South Pole. Two to four maps are placed on a page; this makes it easy to compare the distribution of related species. Thus, on the first page there are four maps (one for each species of the loon), and the specific differences in ranges are immediately comprehensible, with a directness not possible from using the text alone.

The photographs, illustrating 355 of the 419 species, are excellent "shots" from life, which show as much as single pictures can of the habits of each bird.

They are not merely "pretty" pictures, but add to the factual content of the book.

This volume is an English translation (made by the author) of the Dutch version (also published 1960) entitled *Atlas van de Europese Vogels*. The English version has a short preface by A. Lansborough Thomson.

Many Palearctic birds are also found in North America, and their distribution maps include their American ranges; hence, the volume will be of interest to provincial bird students in the United States as well as to others not limited by geographical boundaries.

HERBERT FRIEDMANN
U.S. National Museum,
Smithsonian Institution

La Théorie des Gaz Neutres et Ionisés.

C. DeWitt and J. F. Detoeuf, Eds.
Hermann, Paris; Wiley, New York,
1960. 496 pp. \$17.50.

Owing to new experimental techniques and results and to the extensive application of field-theoretical developments in perturbation theory, statistical mechanics is one of the most exciting fields in physics today. This volume is a high-speed photograph of a rapidly advancing field; in it one can find most of the recent ideas touched upon and a good number of the results that have been obtained by the authors represented.

The book consists of nine articles, of varying length (some articles are in French, the others in English), which review results in kinetic theory, equilibrium statistical mechanics, and plasma physics. Montroll summarizes the development of toron diagrams and their application to the perturbation expansion of the partition function. While this work is most successful for discussing equilibrium properties, applications to transport calculations are also described. Montroll also covers in his article the theory of random walks and some ideas from that theory which are applicable to the Ising problem. Van Hove describes his work in the derivation of the Boltzmann equation from the master equation and in the application of diagrammatic techniques to the elucidation of the long-time behavior of ensembles of interacting particles. This work represents a significant step forward in our under-

standing of how macroscopic physical properties follow from systems of equations which are essentially for functions on the microscopic scale.

These articles are followed by Delcroix's discussion of the microscopic theory of ionized gases in which he relates physical quantities to various distribution functions and describes several common approximations which are useful for plasmas. There are three articles by Kruskal: the first discusses energy principles for equilibrium of a plasma in a magnetic field; the second describes a generalization of the method of Kryloff and Bogoliubov for integrating ordinary differential equations with nearly periodic solutions (an illustrative example would have clarified this work); the third presents an all-too-brief account of the intricacies of Landon damping. The article by Kaufman on plasma transport theory is a long account of results which can be obtained by starting from the Liouville equation and using the more popular of the current approximations in plasma theory. Denisse then discusses some of the elementary and well-known results on the dispersion relations which can be obtained starting from the Boltzmann equation. In the final article, Schatzman applies some of the results of plasma theory to astrophysical data.

This volume is an interesting survey of current problems in statistical mechanics and plasma physics. However, for a photo-offset volume, the price is prohibitive.

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New Books

Biological and Medical Sciences

Aids to Histology. Geoffrey H. Bourne. Baillière, Tindall and Cox, London, ed. 7, 1960 (order from Williams & Wilkins, Baltimore, Md.). 175 pp. Illus. \$3.

Anatomy of the Monocotyledons. 1, Gramineae. C. R. Metcalfe. Oxford Univ. Press, New York, 1960. 792 pp. \$13.45.

The Biotic Associations of Cockroaches. Miscellaneous Collections, vol. 141. Louis M. Roth and Edwin R. Willis. Smithsonian Institution, Washington, D.C., 1960. 476 pp. Illus. + plates. A compilation of material from approximately 1700 sources, including correspondence with other workers.

Biological Activities of Steroids in Relation to Cancer. Gregory Pincus and

Erwin P. Vollmer, Eds. Academic Press, New York, 1960. 546 pp. Illus. \$15. Proceedings of a conference attended by more than 200 scientists, 27 September-2 October 1959; 32 papers by 63 contributors.

Analogue Computers. I. I. Eterman. Translated from the Russian by G. Segal. B. H. Venning, Ed. Pergamon, New York, 1960. 273 pp. Illus. \$8.50.

Biology Code of the Chemical-Biological Coordination Center. vols. 1 and 2. Publ. No. 790. National Academy of Sciences-National Research Council, Washington, D.C., 1960. 230 pp.; 216 pp. \$7 each; \$12 per set. The Code was developed by the Center and was used for storing information on IBM punched cards. Volume 1 contains the series of classification schemes, code items, and symbols; volume 2 describes each of the lists and gives explanations and instructions for use of the code symbols.

Chemobiodynamics and Drug Design. F. W. Schueler. McGraw-Hill, New York, 1960. 652 pp. Illus. \$19.50.

Comparative Biochemistry of Photo-reactive Systems. Mary Belle Allen, Ed. Academic Press, New York, 1960. 449 pp. Illus. \$12.

Developing Cell Systems and Their Control. Dorothea Rudnick, Ed. Ronald, New York, 1960. 246 pp. Illus. \$8.

The Disease Concept of Alcoholism. E. M. Jellinek. Hillhouse Press, New Haven, Conn., 1960. 256 pp. \$6.

The Enzymes. vol. 4, pt. 4, *Hydrolytic Cleavage*. Peptide bond cleavage, other C=N bond cleavage, glycoside bond cleavage, carboxyl ester cleavage. Paul D. Boyer, Henry Lardy, and Karl Myrbäck, Eds. Academic Press, New York, 1960. 651 pp. Illus. \$18.

Faune de France. 64. Isopodes Terrestres. Albert Vandel. Lechevalier, Paris, 1960. 416 pp. Illus. NF. 115.

Fundamentals and Possibilities in Antituberculosis Vaccination. Richard Prigge and Gunther Heymann. Translated by H. Chandler Elliott. Univ. of Toronto Press, Toronto, Canada, 1960. 116 pp. \$5.

Genetics. H. Eldon Sutton, Ed. Josiah Macy, Jr. Foundation, New York, 1960. 229 pp. Illus. \$6. Transactions of the first conference, 19-22 October 1959.

Guide to the Study of the Anatomy of the Shark, *Necturus*, and the Cat. Samuel Eddy, Clarence P. Oliver, and John P. Turner. Wiley, New York, ed. 3, 1960. 141 pp. Illus. \$3.50.

International Review of Cytology. vol. 9. G. H. Bourne and J. F. Danielli, Eds. Academic Press, New York, 1960. 434 pp. Illus. \$13.

Lectures on Haematology. F. G. J. Hayhoe, Ed. Cambridge Univ. Press, New York, 1960. 257 pp. Illus. \$11.50.

Lipide Metabolism. Konrad Bloch, Ed. Wiley, New York, 1960. 424 pp. Illus. \$10.50.

The Mango. Botany, cultivation, and utilization. Lal Behari Singh. Hill, London; Interscience, New York, 1960. 451 pp. Illus. \$13.25.

A Monograph of the Immature Stages of Neotropical Timber Beetles (Cerambycidae). E. A. J. Duffy. British Museum

(Natural History), London, 1960. 335 pp. Illus. + plates. £6 6s. This volume, the third in a series, deals with the Cerambycidae of the Neotropical region: the greater part of Mexico, Central America, the Caribbean and the whole of the South American continent. No attempt is made to deal with strictly North American (Nearctic) Cerambycidae, except when a species extends in distribution as far south as Mexico or occurs in both regions.

The Nature of Animal Colors. H. Munro Fox and Gwynne Vevers. Macmillan, New York, 1960. 256 pp. Illus. \$6.50. An account of animal colors from physical, chemical, and physiological standpoints.

Nematology. Fundamentals and recent advances with emphasis on plant parasitic and soil forms. J. N. Sasser and W. R. Jenkins, Ed. Univ. of North Carolina, Chapel Hill, 1960. 497 pp. Illus.

The Nucleic Acids. vol. 3. Erwin Chargaff and J. J. Davidson, Eds. Academic Press, New York, 1960. 604 pp. Illus. \$18.

Nucleic Acid Outlines. vol. 1, *Structure and Metabolism*. Van R. Potter. Burgess, Minneapolis, Minn., 1960. 300 pp. \$5.

The Organization of Cells and Other Organisms. Laurence Picken. Oxford Univ. Press, London, 1960. 666 pp. 84s.

Phosphorus Metabolism of Brain. P. J. Heald. Pergamon, New York, 1960. 202 pp. Illus. \$6.50.

Plant Disease Handbook. Cynthia Westcott. Van Nostrand, Princeton, N.J., ed. 2, 1960. 832 pp. Illus. \$13.50.

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Principles of Genetics. Eldon J. Gardner. Wiley, New York, 1960. 373 pp. Illus. \$7.50.

Proceedings of the Centenary and Bicentenary Congress of Biology. Singapore, 2-9 December 1958. R. D. Purchon, Ed. Univ. of Malaya Press, Singapore, 1960. 341 pp. Illus. \$13.50. Papers delivered in commemoration of the works of Darwin, Wallace, and Linnaeus.

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Textbook of Pharmacognosy. T. E. Wallis. Churchill, London; Little, Brown, Boston, Mass., ed. 4, 1960. 651 pp. Illus. \$10.

Reports

Transport of Calcareous Fragments by Reef Fishes

Abstract. The weight of sand, coral scrapings, algal fragments, and other calcareous materials which pass through the intestines of reef fishes was calculated on a hectare-per-year basis. It was found that browsing omnivorous reef fishes which rely, in part, on a plant diet ingested and redeposited at least 2300 kg of such material on a 1-hectare study reef near Bermuda. Reasons are presented why this estimate, certainly in order of magnitude, should be applicable to coral reefs in general.

Over a nongeologic time span coral reefs represent relatively stable ecological associations. Live coral colonies can accumulate limestone at remarkable rates (1), while wave and tide erosion as well as animal activity are responsible for the breakdown of the dead coral. Burrowing annelid worms, mollusks, and crustaceans riddle the coral rock and weaken it for the attack of mechanical forces, while some fishes either scrape on corals (parrot fishes, Scaridae) or nibble on coral branches (filefishes, Monacanthidae). Many calcareous algae occur on the reef with its calcium-rich water, and these are eaten by certain reef fishes (surgeonfishes, Acanthuridae, and others). Coralline and foraminiferous sands are also frequently found in large quantities in the digestive tracts of many families of reef fishes (2). These materials may be eaten by the fishes for various reasons; some fragments are certainly ingested accidentally in the quest for

small attached animals and algae; shells are crushed and swallowed, but, at least in the case of parrot fishes and surgeonfishes, sand serves as a milling agent for algal cell walls and perhaps even contributes to the buffering of intestinal juices.

I have attempted to arrive at an estimate of the amount of calcareous material, shell fragments, coral scrapings and branches, coralline algae, and sand that passes through the digestive tract of reef fishes to be redeposited for further displacement by waves or tides. My calculations and observations indicate that between 2 and 3 metric tons of calcareous material per hectare are redeposited on a typical Bermuda reef after passage through the digestive tract of fishes. Cloud (3) mentions that several tons of sand and fine gravel per hectare (1.100 to 1.600 metric ton/m²) are moved by fishes on Pacific atolls but cautions that this figure is based on some assumptions and wholesale estimates; there are no methods given.

Atlantic and Pacific reef-fish faunas are reasonably alike, at least in the feeding adaptations of their members, and estimates of total fish biomass for Bermuda and for certain Pacific reef environments are also comparable (4); it may therefore be safe to extend my estimates to reefs in general, at least in order of magnitude.

Estimates of the amounts of calcareous material (not differentiated as to type or provenience) which went through the fishes were reached as follows.

1) The stomachs and intestines of many reef fishes were examined for calcareous material.

2) The numbers of individuals of different species present were taken from a standing-crop estimate on a 1-hectare Bermuda reef (4).

3) The digestive tracts of about 200 individuals were weighed wet and oven-dried; full and empty intestines were used, to arrive at the relative weight of the gut and its contents. Water content of 75 to 80 percent was assumed for algal and invertebrate remains.

4) The passage time of materials through the intestine was determined by introducing a small amount of charcoal or vital dye with a blunted large injection needle past the pharynx of each fish while it was under MS-222 anesthesia. After separation of different species and size groups into various tanks, the time of the first appearance of colored feces was recorded. These passage times were checked against the time of natural emptying when the fishes were not fed.

5) Scuba and skindiving observations were made, in the field, of the time certain fishes spent in feeding during periods of daylight.

Thus, from knowledge of the numbers of coral- and sand-ingesters present, their rate of feeding and digestion, and the weight of calcareous fragments in the gut at any one time, a fair estimate of yearly deposition of such materials could be made.

The following families, given roughly in the order of their importance, were, in the main, found to carry sand, coral fragments, and other calcareous matter in their stomachs and intestines [see also Hiatt and Strasburg (2)]: parrot fishes (Scaridae) and surgeonfishes (Acanthuridae) (consistently large amounts of sand, coral fragments, and limestone powder present); butterfly fishes (Chaetodontidae) and damselfishes (Pomacentridae); gobies (Gobiidae); triggerfishes (Balistidae); goatfishes (Mullidae); wrasses (Labridae); and puffers (Tetraodontidae) (frequent to occasional findings of the above materials).

Small fishes and juveniles (10 to 50 g) passed colored fecal pellets after 1 to 2 hours, while medium-sized omnivores which feed predominantly on algae (250 to 500 g) retained ingested food for 3 to 5 hours—findings which indicate that the gut is filled thrice daily, or more often, in small fishes and twice daily in the medium-sized group. Fishes weighing over 1000 g are not likely to turn over their intestinal content more than once a day. Parrot fishes and surgeonfishes spent from two-thirds to four-fifths of their time feeding when they were observed during 5- to 10-minute periods scattered throughout the day.

There were 55 kg of small and young browsing fishes per hectare on the Bermuda study reef; they belonged to all the groups listed above with the exception of the triggerfishes (Balistidae). The dry, nonnutritive material found in their intestines amounted to between 2 and 4 percent of their wet body weight, and they turned over their food at least three times a day.

If they are fully active for 8 months

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should not repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to one 2-column figure (that is, a figure whose width equals two columns of text) or to one 2-column table or to two 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [Science 125, 16 (1957)].

(240 days), if they have three feeding cycles a day, and if calcareous fragments in the gut constitute 3 percent of the body weight, then the fishes must redeposit 1080 kg (roughly 1 ton) of calcareous material per hectare per year (3 percent of 55 kg \times 3 \times 240).

Analogous calculations have indicated that one or two daily fillings of the digestive tracts yield about 700 kg of material per hectare per year from surgeonfishes and 600 kg from parrot fishes. The larger filefishes were disregarded because they were present in relatively small numbers on the study reef. Where they occur in greater numbers they should be included in such an estimate because they feed almost exclusively on the tips of branching corals (2).

Adding the weight of calcareous material, purposely or accidentally ingested, which passes through the gut of small, medium-sized, and larger browsing reef fishes, one arrives at a total weight of at least 2300 kg/hectare yr.

Possible sources of error in this study lie in the following factors.

1) The estimate of numbers of fishes present. This source of error was discussed in a previous publication (4), where it was suggested that the estimate was on the low rather than on the high side.

2) The indicated effect of temperature on feeding. While such effects may be considerable in Bermuda, where corals and certain reef fishes live at the northern margin of their range, they should not be noticeable in truly tropical circumequatorial reef regions.

3) The omission of certain additional groups of fishes which only rarely ingest sand and coral or calcareous algal fragments (for example, jacks, Carangidae). For Bermuda this omission would tend to counteract an error on the high side resulting from reductions in feeding during the winter months, but for more tropical regions the omission would make my estimate of the amount of redeposited material too low.

4) The occurrence of periods of non-feeding—for instance, during spawning. I do not believe this to be a large consideration because (i) parrot fishes were observed to feed in the spawning season, on one occasion even between successive pairing acts, and (ii) the nutritive content of attached algae forces fish which feed largely or partly on such materials to take in substantial quantities of food to sustain themselves, to say nothing of growing (5, 6).

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13 JANUARY 1961

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- This study was carried out at the Bermuda Biological Station for Research and at the Lerner Marine Laboratory at Bimini. I thank the directors and staffs of both stations for help received. The investigation was supported by the Horace Rackham School of Graduate Studies of the University of Michigan and by the Lerner Marine Laboratory. This report is contribution No. 277 from the Bermuda Biological Station for Research.

19 September 1960

Facilitation of Infection of Monkey Cells with Poliovirus "Ribonucleic Acid"

Abstract. The plaque titer of poliovirus "ribonucleic acid" on monkey kidney cells cultured *in vitro* is greatly increased by depleting these cells of calcium and treating the "ribonucleic acid" inoculum with a suspension of any one of several poorly water-soluble substances before inoculation. These undissolved substances apparently facilitate infection by serving as solid vehicles for the "ribonucleic acid."

Intact ribonuclease-stable poliovirus is changed by phenol into an infective unit destructible with ribonuclease; this ribonuclease-labile poliovirus unit is called poliovirus "ribonucleic acid (RNA)" (1). When standard plaque assay techniques are used poliovirus "RNA" manifests a plaque titer on monkey kidney tissue cultures of only about $10^{-6.5}$ of the titer of the intact virus from which it was prepared (2). In this report we show how the titer of poliovirus "RNA" on such kidney cells can be greatly increased (3).

Poliovirus "RNA" was obtained by one treatment of intact wild-type virus of the Brunhilde strain (antigenic type 1) at 0° with 7 percent water-saturated phenol (2); this method is a modification of method of Gierer and Schramm (4). Kidney cells were grown and maintained and poliovirus stocks were obtained as described previously (2).

The combination of two specific procedures results in a large increase in the number of plaques produced by poliovirus "RNA." These two procedures are (i) addition to the "RNA" of any one of several compounds of low solubility in water, and (ii) depletion of the kidney cells of calcium (Table 1). Poorly soluble substances which facilitate infection of calcium-depleted cells with "RNA" include, besides $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ (5) and Cr_2O_3 (Table 1), Al_2O_3 , CaCO_3 , CaSO_4 , Co_2O_3 , Fe_2O_3 , Fuller's earth, MgCO_3 ,

MgF_2 , $\text{MgHPO}_4 \cdot 3\text{H}_2\text{O}$, $\text{Mg}_3(\text{PO}_4)_2 \cdot 5\text{H}_2\text{O}$, $\text{Mg}_3\text{Si}_4\text{O}_{11} \cdot \text{H}_2\text{O}$, NiO , and ZnS . Two of these facilitators were tested for their capacity to adsorb poliovirus "RNA": at a facilitator concentration of 0.25 percent, $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ adsorbed 56 percent and $\text{Mg}_3(\text{PO}_4)_2 \cdot 5\text{H}_2\text{O}$ adsorbed 67 percent of the "RNA."

With $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ as facilitator, and calcium-depleted cells, the number of plaques formed is dependent on the tonicity both of the medium used for the "RNA" and of the medium used for the cells (Table 1) (see 6). When the medium for the cells is isotonic fewer plaques are formed when the medium for the "RNA" is hypertonic than when it is isotonic or hypotonic. When the medium for the "RNA" is slightly hypotonic, peak plaque production is obtained when the medium for the cells is slightly hypotonic.

With $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ as facilitator, and calcium-depleted cells, the number of plaques produced by the "RNA" was dependent on the duration of the post-inoculation incubation at 37°C before layering with the nutritional agar maintenance medium. The results of three experiments suggest that the largest number of plaques is obtained when this duration approximates 1 hour. The moderately large variation among these experiments, however, suggests

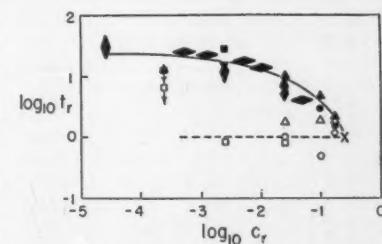


Fig. 1. The relationship of calculated plaque titer of poliovirus "RNA" with "RNA" concentration inoculated. Symbol " c_r " means relative concentration of "RNA" inoculated, based on assigning unity concentration value to "RNA" concentration in undiluted "RNA" preparation. Symbol " t_r " means calculated relative titer of "RNA," based on assigning unity titer value to titer obtained when $c_r = 0.25$. For $c_r = 0.25$, denoted by X , and for all solid symbols inoculum contained 0.25 percent $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$; for all open symbols inoculum contained < 0.25 percent $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ since these inocula were obtained by dilution of the mixture of "RNA" and $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ into diluent(s) without $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$. All symbols of same shape from same experiment, of different shape from different experiments. Arrows denote maximal points; that is, no plaques were found at inoculum "RNA" concentration indicated.

the importance of variation among kidney cell preparations.

The calculated plaque titer of the "RNA" plus $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ on calcium-depleted cells is independent of "RNA" concentration inoculated if the subsequent dilutions are made into diluent without facilitator but dependent on inoculum concentration when the diluent contains facilitator (Fig. 1). With 0.25 percent $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ in subsequent diluents, as the inoculum "RNA" concentration is decreased, the calculated plaque titer increases and then asymptotes at a value about twenty-five times the titer obtained when the inoculum "RNA" concentration is at the high value of 0.25. Without facilitator in subsequent diluents, the calculated titer, though independent of "RNA" concentration inoculated, is, strictly speaking, *not an "RNA" titer*.

but is a titer of $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$, since the number of plaques produced is limited by the concentration of this phosphate.

Poliovirus "RNA" facilitated by 0.25 percent $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ and inoculated at relatively low "RNA" concentrations onto calcium-depleted kidney cells titers about 10^{-3} of the titer of the intact virus from which it was prepared. It may be possible to raise this relative titer of the "RNA" still further by (i) more severe depletion of the kidney cell of its calcium or (ii) the use of some facilitator other than $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$, perhaps at a higher concentration, or both (i) and (ii).

Poliovirus "RNA" very probably lacks, wholly or partially, the natural cell-entry mechanism possessed by intact poliovirus. A sensitive system for the biological assay of "RNA" then

probably must provide a substitute mechanism, which could derive from increasing either the receptivity of the cell or the efficaciousness of the "RNA," or both. We have increased both for providing what is probably a substitute cell-entry mechanism for the poliovirus "RNA" (7).

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5. Infection is also facilitated by the fine cloudy precipitate, very probably a calcium phosphate, formed when phosphate-buffered saline is made by mixing its ingredients before sufficient dilution with water.
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Table 1. The effects of varying four facets of the environment on the number of plaques produced by poliovirus "ribonucleic acid" ("RNA") on monkey kidney tissue cultures. Horizontal spaces separate experiments within which "RNA" concentration in inoculum was constant. Tonicity of media are expressed relative to that of 0.154M NaCl (I).

Tonicity of medium for "RNA"	Poorly soluble compound added to "RNA"†	Tonicity of medium for cells‡	Cation of which cells are depleted§	No. of plaques per plate*	
				Individual plates	Arithmetic mean
0.90 I	None	I	None	1;1	1.0
0.90 I	None	4.4 I	h	0;3	1.5
4.0 I	None	I	None	2;2;5	3.0
4.0 I	None	4.4 I	h	0;0;0	<0.3
0.75 I	None	I	None	3;0;1	1.3
0.75 I	P	I	None	11;8;10	9.7
0.75 I	None	I	None(TT)	0;0;0;1;1	0.4
0.75 I	None	I	Ca ⁺⁺	0;2;0;0	0.5
0.75 I	None	I	Mg ⁺⁺	0;1;0;0	0.2
0.75 I	None	I	Ca ⁺⁺ and Mg ⁺⁺	0;0;0;0	<0.2
0.75 I	P	I	None(TT)	1;3;1;1	1.5
0.75 I	P	I	Ca ⁺⁺	30;27;21;34	28.0
0.75 I	P	I	Mg ⁺⁺	11;6;2;5	6.0
0.75 I	P	I	Ca ⁺⁺ and Mg ⁺⁺	27;23;29;17	24.0
0.25 I	P	I	Ca ⁺⁺	37;23;16;29;22;13	23.3
0.35 I	P	I	Ca ⁺⁺	44;24;28;42;30	33.6
0.75 I	P	I	Ca ⁺⁺	26;21;33;22;48;9	26.5
2.8 I	P	I	Ca ⁺⁺	4;6;6;12;5;5	6.3
6.7 I	P	I	Ca ⁺⁺	5;1;3;4;5;3	3.5
0.75 I	P	0.17 I	Ca ⁺⁺	3;1;3;4;4	3.0
0.75 I	P	0.50 I	Ca ⁺⁺	22;24;23;30	24.8
0.75 I	P	I	Ca ⁺⁺	23;22;36;22;26	25.8
0.75 I	P	2.0 I	Ca ⁺⁺	9;13;9	10.3
0.75 I	P	5.0 I	Ca ⁺⁺	2;1;6;0;1	2.0
0.75 I	None	I	Ca ⁺⁺	1;1;0	0.7
0.75 I	R, 0.012%	I	Ca ⁺⁺	5;6;3	4.7
0.75 I	R, 0.50%	I	Ca ⁺⁺	55;52;36	47.7
0.75 I	R, 1.0%	I	Ca ⁺⁺	160;84;100	114.7
0.75 I	R, 1.5%	I	Ca ⁺⁺	63;117;84	88.0

* 0.30 ml inoculated per plate.

† "P" means 0.25 percent $\text{CaHPO}_4 \cdot 2\text{H}_2\text{O}$ in the inoculum. "R" means Cr_2O_3 ; the percent values are concentrations of Cr_2O_3 in inoculum.

‡ The two values "4.4 I" are for the hypertonic medium (HM) described by Mountain and Alexander (8).

§ Only the metallic cations present in the phosphate-buffered saline (PBS) used by Dulbecco and Vogt (9), namely Na^+ , K^+ , Ca^{++} , and Mg^{++} , are here considered. Cells were depleted by washing with PBS without either Ca^{++} or Mg^{++} or without both, incubating $\frac{1}{2}$ hour at 37°C , and rewashing with the kind of medium used for the first washing. Many of the cells depicted in this way of both these divalent cations or of Ca^{++} alone are rounded. The designation "None(TT)" indicates "time-temperature" control cultures whose cells were treated the same as the depleted cells except that PBS was used. The designation "None" indicates cultures which were washed with PBS. Designation "h" indicates cultures which were washed with PBS, incubated in HM for $\frac{1}{2}$ hour at 37°C , and inoculated after removal of HM but without rewashing; these cultures may have been partially depleted of one or more of the cations K^+ , Ca^{++} , and Mg^{++} .

|| HM was the main component.

Cytochrome c Reductase of Tri- and Diphosphopyridine Nucleotides in Rat Lens

Abstract. The ocular lens of the 28- to 32-day-old rat contains an active hexose monophosphate shunt pathway for the combustion of glucose. Triphosphopyridine nucleotide (TPNH) cytochrome c reductase is present in this organ and is approximately one-third more active than diphosphopyridine nucleotide (DPNH) cytochrome c reductase. Since there is no transhydrogenase activity in these lenses, and since DPNH lactic dehydrogenase is 15 times as active as TPNH lactic dehydrogenase, the presence of an active TPNH cytochrome c reductase may provide this organ with the means of reoxidizing the relatively large amounts of TPNH formed by the direct oxidative pathway of glucose metabolism. Although TPNH oxidation in other tissues has not as yet been shown to yield adenosine triphosphate (ATP) directly, it is possible that such a mechanism may be operative in the rat lens.

Previous experiments have indicated that the hexose monophosphate shunt may be an important pathway of carbohydrate metabolism in the lens of the young rat (1). Recent studies to determine the levels of oxidized and reduced di- and triphosphopyridine nucleotides (DPN, TPN, DPNH, and TPNH) have shown that the TPNH:TPN ratio in the

lens derived from the young rat (aged 28 to 32 days) is 5.69 as compared with a DPNH:DPN ratio of 1.52. (2) These results provide further evidence of an active shunt pathway in this organ.

There must be some mechanism within the lens to oxidize the relatively large amounts of TPNH formed during the first two steps of glucose oxidation via the shunt. Employing the method of Kaplan *et al.* (3) and Stein *et al.* (4) in which DPN analogues are used, I was unable to demonstrate by repeated assays any transhydrogenase activity within the rat lens. Although a relatively inactive TPNH-linked lactic dehydrogenase is present in this organ, its activity is only approximately 1/15 the activity of DPN lactic dehydrogenase (5) at pH 7.4, and it is difficult to see how such a system could account for an efficient and rapid reoxidation of TPNH.

TPNH and DPNH cytochrome c dehydrogenase were then looked for in the lens. Pirie *et al.* (6) have reported that TPNH cytochrome c dehydrogenase activity is present in the rat lens. Employing their assay system, I discovered that cytochrome c could be reduced very rapidly if only an aliquot of a lens homogenate—and no TPNH or DPNH—was added to the cuvette. Four lenses derived from 28- to 32-day-old Holtzman strain white male rats were therefore homogenized in 5 to 10 times their weight in deionized water, dialyzed overnight, and centrifuged; and the supernatant solution was retained for the assay procedure. Since an aliquot of this supernatant solution was also capable of rapidly reducing cytochrome c (without any added DPNH or TPNH), the solutions were assayed for any ascorbate that might still be present in a bound form. The Roe procedure based on the reduction of the dye 2,6 dichlorophenol-indophenol (7) was employed as a simple assay for ascorbate, and the results showed that no ascorbate was present in these dialized solutions. There were also no sulfhydryl groups present (for example, reduced glutathione or cysteine) in sufficient quantity to account for the degree of cytochrome c reduction. Heating the solution for 2 minutes at 100°C completely destroyed its activity. Studies are now in progress to further characterize and determine this compound.

In order to assay for TPNH and DPNH cytochrome c activity, it was thus necessary to add a large excess of cytochrome c (type III Sigma) to the test system and allow the initial reduction of cytochrome c to go to completion. Approximately 30 to 45 seconds were required for this to occur. Either TPNH or DPNH was then added to the cuvette, and further reduction of cyto-

Table 1. Results of assays for TPNH and DPNH cytochrome c reductase activity. The enzyme activity is expressed as the change in optical density at 550 m μ of 0.001 per minute at 24°C per lens.

Lenses (No.)	Av. wet weight per lens (mg)	Cytochrome c reductase activity	
		TPNH	DPNH
4	19.10	14.40	6.91
4	19.20	17.75	14.40
4	19.02	19.72	14.79
4	19.60	19.72	14.709
Mean activity		17.90	12.72

chrome c was determined for a 5-minute interval. The results of such assays on four samples obtained from 16 rat lenses for TPNH and four samples (16 lenses) for DPNH cytochrome c reductase activity are shown in Table 1. Both TPNH and DPNH cytochrome c reductases are present within the rat lens, and the former is apparently about 30 percent more active (8).

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26 May 1960

Estimation of Total Body Fat from Potassium-40 Content

Abstract. On the assumption that the potassium content of the lean body mass is constant, it should be possible to estimate fat content in living man from a measurement of potassium-40 activity in the whole-body scintillation counter. A series of such measurements on children and young adults shows good correlations with skin-fold thickness and weight/height ratio as indices of fatness.

Current interest in obesity has resulted in attempts to assess the actual fat content of the body in living human subjects. Methods now in use include calculations based on measurement of total body water and body density (1) as well as inferences drawn from

measurements of skin-fold thickness. The assumptions involved in the first two methods, and their inherent limitations, have recently been discussed (2).

The purpose of our report is to suggest a new approach to this problem, namely the use of whole-body potassium content as an index of lean body mass. This approach is based on results of chemical analyses of adult human subjects; only four such analyses have been done, and these revealed values of 66.5, 66.6, 72.8, and 66.8 meq/kg of lean body weight (3) (this term is taken to mean body weight minus chemically determined neutral fat). Other workers have emphasized the relative constancy of whole-body potassium content in such species as the cat, rabbit, rat, and pig when values are expressed on a fat-free basis (4), though the absolute values are somewhat higher than those for man. There is a high correlation between K⁴⁰ content and lean tissue mass in hams (5). Woodward *et al.* (6) found a good correspondence between K⁴⁰ content and lean body mass as determined by tritium dilution in man. Meneely *et al.* (7) were able to correlate K⁴⁰ activity with basal heat production. The assumption of a constant potassium content for the lean body mass would thus appear to be a reasonable one.

Fifty subjects were accordingly assayed for K⁴⁰ in a whole-body scintillation counter, and the estimates of fat content so derived were correlated with skin-fold thickness and weight/height ratio.

The subjects comprised 42 males, aged 11 to 44 years, and 8 females, aged 7 to 23 years. Body weights ranged from 22 to 105 kg. All were judged to be in good physical health. An attempt was made to include both obese and thin subjects so the results cannot be taken as representative of the normal population. Subjects were dressed only in a light cotton gown and paper slippers at the time of measurement. Skin-fold thickness was determined at six locations using special calipers (8): mid-biceps, mid-triceps, abdomen below and 1 cm to left of umbilicus, subcostal at nipple line, iliac crest, and subscapular.

Potassium-40 measurements were made in a whole body scintillation counter, according to the technique described in detail by Miller (9). The subject reclines in a specially constructed metal chair and is viewed by an 8-inch diameter, 4-inch thick sodium iodide crystal. The gamma rays emitted by the subject, which penetrate the crystal, produce scintillations which are seen by four 3-inch photo multiplier tubes. The signal output from the pho-

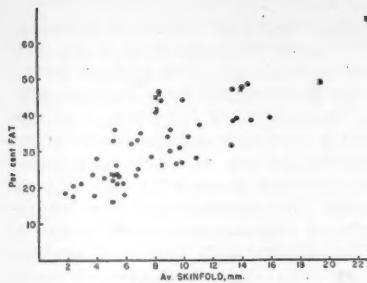


Fig. 1. Percentage of fat in body versus average skin-fold thickness. Females (x), males (•).

tomultipliers goes to the spectrum analyzer dropping into any one of 100 channels depending on the pulse amplitude. Potassium-40 is measured by counting the pulses in the photopeak for a measurement time of 30 minutes. Background interference is reduced by placing the subject and the crystal detector in a room with 8-inch thick steel walls. The system has been calibrated by the measurement of laboratory personnel of a selected size range who ingested known amounts of potassium-42. The sensitivity and reproducibility are such that remeasurement of the same individual gives the same result with a standard deviation of about 1.5 percent. Stable potassium content can be readily calculated from the normal abundance of K^{40} .

Fat content was calculated as the difference between total weight and lean body weight (LBW), on the basis that the latter has a potassium content of 68.1 meq/kg:

$$LBW \text{ (kg)} = \frac{\text{measured total K (meq)}}{68.1}$$

This equation is analogous to that in common use for determination of LBW by deuterium or tritium dilution: namely, $LBW = \text{total body water}/720$, where the denominator is the water content per kilogram of LBW.

The range of potassium content for

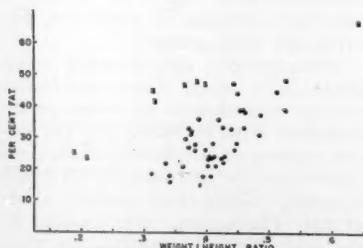


Fig. 2. Percentage of fat versus weight/height ratio (kg/cm). Females (x), males (•).

our subjects was 35-58 meq/kg in the males, and 23-52 meq/kg in the females. These values compare favorably with those reported by Anderson and Langham (10) in a much larger group of subjects studied at Los Alamos. Calculated fat content was 16 to 48 percent of total body weight in males and 24 to 67 percent in females. These figures support our clinical impression that some of the subjects were obese.

The relationships between fat content as determined by K^{40} measurement and two other parameters of fatness-leanness are depicted in Figs. 1 and 2. The correlation coefficient of fat content against average skin-fold thickness is 0.80 for the males, and that of fat content against weight/height ratio is 0.56 (males only). The data on females are too few to justify calculation of correlation coefficients for this group. However, the graphs suggest that the trends are similar to those of the males. It is of interest that females tend to have a higher fat content for a given weight/height ratio than do the males.

This report is presented as a new approach to the estimation of fat content in living man. The procedure has the obvious advantage of being nontraumatic and devoid of hazard to the subject. Work is now in progress in a further attempt to assess the validity and accuracy of this method (11).

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29 September 1960

Effect of Deuterium Substitution in Sympathomimetic Amines on Adrenergic Responses

Abstract. It was discovered that replacement of the α -hydrogens of tyramine and tryptamine by deuterium produces a marked intensification of the blood pressure effects and nictitating membrane contraction normally produced by these amines. The results are interpreted on the basis of kinetic isotope effects at the level of monoamine oxidase and clearly establish the importance of this enzyme in the limitation of responses when tyramine and tryptamine are involved. The observed deuterium isotope effects with α,α -bis-deuterotyramine (α,α -D-tyramine) have been reproduced with only one of the optical isomers of monodeuterotyramine. This establishes that the enzyme displays a high degree of optical specificity. The use of *l*-bisdeuteronorepinephrine revealed that norepinephrine cannot be attacked by the enzyme at the effector cell level.

The role of monoamine oxidase (MO) in relation to adrenergic mechanisms has long been a matter of some controversy (1). It was suggested some time ago (2) that the excitatory properties of drugs such as amphetamine or ephedrine were at least partly related to their inhibitory properties towards monoamine oxidase. In more recent years, the discovery of potent new inhibitors has made it possible to establish an important role for this enzyme in adrenergic mechanisms (3). Thus, the administration of iproniazid could be shown (4) to potentiate the action of various adrenergic amines, such as tyramine, on the nictitating membrane of the cat. However, no potentiation of the action of the normal transmitter norepinephrine could be demonstrated (4). This would seem to indicate that termination of the action of norepinephrine and epinephrine does not involve breakdown through attack by monoamine oxidase. It has been shown by Axelrod and his collaborators that O-methylation is in all probability the mechanism responsible for inactivation of catecholamines (5) although, according to Brodin and his group, monoamine oxidase may be involved in the disposition of catecholamines in brain (6). However, the demonstration by Carlson (7) of the presence of appreciable concentrations of dopamine in brain (as well as serotonin) provides a possibility that the central effects of iproniazid could be related in some way to increased levels of dopamine and serotonin (6), both of which can act as substrates for monoamine oxidase.

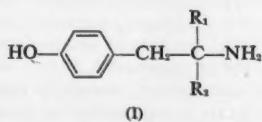
It has not yet been possible to assess quantitatively and unambiguously the role of monoamine oxidase in adrenergic mechanisms because of the use of large doses of noncompetitive inhibitors

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which create conditions that are far from physiological. Moreover, these drugs lack specificity (8) (analgesia, hypoglycemia, hypotension, and so forth), and it is likely that in their presence the physiology of adrenergic effector cells is altered (1), thus making the interpretation of results difficult.

We have discovered a new approach, which is free of ambiguities, to the problem of the physiological role of monoamine oxidase in adrenergic mechanisms. We wish to report at this time some of the most significant preliminary results which best illustrate the principles involved in this approach.

It was reasoned that, in order to evaluate monoamine oxidase activity in effector cells and other tissues, it would be essential to compare two adrenergic amines stereochemically indistinguishable by monoamine oxidase but differing in susceptibility to degradation by it. Such requirements are not met by the usual series of analogs, isologs, or isosteres because of distinct stereochemical differences between any two compounds. However, the substitution of deuterium for hydrogen atoms in a molecule does fulfill the above requirements, and differences between the rate of degradation of a substrate and its deuterium-labeled counterpart are to be expected if the rate-limiting step of the degradation involves breaking of a carbon-hydrogen bond. As an example, should the adrenergic amine tyramine (which is a good substrate for monoamine oxidase) be labeled with deuterium on the α -carbon ($I, R_1 = R_2 = D$), the rate of its oxidation by the enzyme should be decreased as long as the rate-determining step involves breaking of a carbon-deuterium bond.



Now, if monoamine oxidase is involved as a limiting factor in the response of a nictitating membrane, the substitution of deuterium for hydrogen in tyramine should lead to quantitative differences in the pattern of membrane contraction. Since the labeled tyramine is stereochemically identical to normal tyramine, any difference in response can be ascribed to variations in binding constants and rate of degradation of the substrate under perfectly controlled physiological conditions. Similar reasoning also applies to norepinephrine which in a similarly labeled form might lead to valuable information about the nature of receptors.

Results. Pentobarbitalized cats were prepared for recording blood pressure

Table 1. Comparison of tyramine and α,α -bisdeuterotyramine on sympathetic receptors in the cat.

	No. tests	Intravenous dose ($\mu\text{g}/\text{kg}$)	Mean maximum blood pressure increase (mm-Hg)	Mean pressor area*	Mean nictitating membrane contraction	
					Maximum (mm)	Area ₅₀ *†
Tyramine	17	800	82	142	11	54
α,α -D ₂ -tyramine	9	800	84	291	22	179
Ratio: α,α -D ₂ -tyramine / tyramine				2.1	2.0	3.3
<i>p</i>			0.75	<0.001	<0.001	<0.001

* Areas determined with planimeter

† Areas under curve to 50-percent recovery.

and nictitating membrane contractions in the conventional manner. Tyramine and α,α -bisdeuterotyramine (98 percent isotopic purity) ($I, R_1 = R_2 = D$) were administered intravenously in equivalent doses. Figure 1 illustrates the effects of the two materials on arterial pressure and membrane contraction in typical experiments. Table 1 summarizes the results of all studies carried out. The magnitude of the pressor response was essentially the same for both compounds, but the duration of the effect (reflected in pressor area) was twice as prolonged with α,α -D₂-tyramine (statistical analysis giving a *p* value < 0.001). Both the magnitude and duration of the nictitating membrane contractions were twice as large after administration of α,α -D₂-tyramine as with tyramine (*p* < 0.001). The greater magnitude of con-

traction observed in the latter instance is probably related to the lack of compensatory mechanisms such as exists for blood pressure maintenance. When α,α -bisdeuterotryptamine was compared with tryptamine under the same conditions, an identical pattern of differences was observed. However, when *l*-bis- α -deuteronorepinephrine (synthesis to be described elsewhere) was compared with *l*-norepinephrine, no difference whatsoever could be observed in their potencies [see Fig. 1 (1A, 1B)].

Stereospecificity of the deuterium isotope effects. In view of the above results, it was of considerable interest to examine the possibility that the observed isotope effects may be stereospecific, that is, assuming a three-point contact between substrate and enzyme or receptor, only one of the two possible

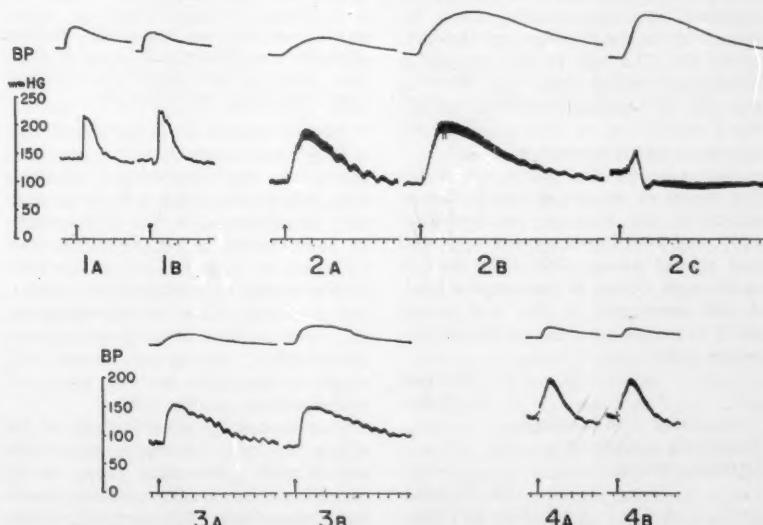


Fig. 1. Effects of tyramine and α,α -bisdeuterotyramine on arterial pressure and membrane contraction. Female cats (3.0 to 3.5 kg), under pentobarbital anesthesia, were used. From top to bottom: isotonic contraction of nictitating membrane, arterial blood pressure, time in minutes. Test compound given at arrows (intravenously): norepinephrine, 16 $\mu\text{g}/\text{kg}$, all tyramine compounds, 800 $\mu\text{g}/\text{kg}$. 1. Cat 3.1 kg: *A*, *l*-norepinephrine; *B*, *l*- α -bisdeuteronorepinephrine. 2. Cat 3.0 kg: *A*, tyramine; *B*, α,α -bisdeuterotyramine; *C*, α -monodeuterotyramine (prepared enzymically). 3. Cat 3.5 kg: *A*, tyramine; *B*, α -monodeuterotyramine (same isomer as in 2C but prepared synthetically). 4. Cat 3.2 kg: *A*, tyramine; *B*, α -monodeuterotyramine (opposite isomer of that shown in 2C and 3B, prepared enzymically).

optical isomers of α -monodeuterotyramine (**I**, $R_1 = H$; $R_2 = D$; and **I**, $R_1 = D$; $R_2 = H$) may be responsible for the isotope effect observed with the bisdeutero amine. Both optical isomers of α -deuterotyramine were prepared enzymically from tyrosine (9) and assayed as above. The results are shown in Fig. 1 (2C and 4B) (10) and clearly establish that the isotope effect is completely stereospecific in accordance with a three-point contact between tyramine and monoamine oxidase. The absolute dependence on configuration of the isotope effect on the initiating membrane response fully agrees with our deductions based on *in vitro* studies with liver monoamine oxidase (9).

Conclusions. From these observations, the following conclusions emerge: (i) The enzyme monoamine oxidase must be intimately associated with adrenergic effector cells and must be an important factor in the limitation of the action of tyramine and tryptamine. (ii) The monoamine oxidase involved in adrenergic mechanisms displays an absolute stereospecificity which is identical to that of liver monoamine oxidase, thus making it probable that these two enzymes are very similar in properties and mechanism of action. (iii) Norepinephrine is not a substrate for the enzyme at the adrenergic effector cell level. This excludes an oxidative deamination of transmitter as part of the sequence of events leading to a response or to inactivation of the substrate. (iv) The role of the enzyme in adrenergic mechanisms can best be pictured as a protective device for the rapid disposition of circulating or endogenous nontransmitter material.

So far as we are aware, this is the first report on the use of kinetic isotope effects in the field of pharmacology (11) and constitutes a novel approach that should prove a powerful tool in mechanism studies at the receptor level. A full description of this and related work in progress will be published elsewhere (12).

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10. The unusual blood pressure effect of the enzymically prepared tyramine (Fig. 1, 2C) has been traced to the presence of a minute amount of a phenolic impurity which is formed when oxygen is not excluded from the incubation mixture. A purely synthetic sample of optically active α -D-tyramine produced a typical blood pressure response (Fig. 1, 3B).
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5 August 1960

Effects of Amino Acid Feedings in Schizophrenic Patients Treated with Iproniazid

Abstract. Large oral doses of individual amino acids were given three or four times daily for periods of 1 week to schizophrenic patients, some of whom were maintained on iproniazid. Marked alterations in behavior in some patients were associated with the administration of L-methionine and of L-tryptophan.

Recent reports have indicated that certain amino acids or their endogenous derivatives may affect mental state and behavior, and have led to hypotheses of their involvement in the pathogenesis of some forms of schizophrenia (1). Evidence for such hypotheses has been sought in the psychotomimetic properties of certain amines or their congeners (2), and in qualitative or quantitative abnormalities among the amino acid products excreted in the urine of schizophrenic patients (3).

This report describes a study of the effects of large quantities of certain amino acids given to a group of 12 chronic schizophrenic patients, previously described (4), nine of whom were also given iproniazid throughout the study in an effort to increase tissue concentrations of endogenous amines. None had received somatic therapies within the previous 18 months.

The study is summarized in Table 1. There were three time blocks (periods A, B, and D) during which, in a Latin square design, every patient received in rotation each of the tabulated amino

acids (or amino acid combinations) for approximately 1 week. During periods C, E, and F the Latin square design was not used. Instead, certain individuals received only one of the substances tabulated, selection depending primarily on their having previously responded to the same, or a related substance. Except as noted in Table 1, the L forms of amino acids were given, suspended in chocolate milk, three times daily. The appearance, consistency, and taste of the suspensions were made as similar as possible by adding barium sulfate or flavoring agents as needed.

Behavioral observations were made continuously by nursing personnel. Each patient was evaluated daily by one or more of three psychiatrists. Three additional physicians together examined the patients at weekly intervals and were the only participants aware of the drug and amino acid regimens. All patients were observed for signs of hepatic disturbance, and serum transaminase was measured each week. No hepatic dysfunction was detected. Electroencephalographic, psychometric, and biochemical studies, which complemented the behavioral and psychiatric evaluations, are in preparation.

Marked behavioral changes occurred with L-methionine loading in four of the nine patients receiving iproniazid—in two patients on each of three trials, in one on two of three trials, and in one on one of two trials. The major clinical features in these patients were an increasing flood of associations often reaching "word salad," increasing anxiety approaching or reaching panic, increasing tension and motor activity, depression accompanying a brief period of sharply increased insight, an upsurge of hallucinatory activity, and brief intermittent periods of disorientation at the height of agitation.

One such patient, essentially mute for many years, manifested a flood of speech and ideas that was uncontrollable for hours. Another patient, paranoid but usually coherent and in good contact, described a flood of ideas, had a sudden depressed insight into the effects of his psychosis on himself and those about him, was flooded with associations, reached the stage of "word salad," and finally repeated isolated letters continuously.

Most changes disappeared abruptly upon withdrawal of methionine, but concurrently each marked reactor was thought to show some unexpected clinical improvement which persisted for weeks or months. The dose level of iproniazid did not seem to influence the intensity of methionine effects.

The changes cannot be attributed to the metabolic acidosis or persistent gas-

Table 1. Behavioral change in chronic schizophrenic patients maintained on iproniazid and loaded with amino acids or related substances.

Weeks of study	Load period	Loading substance and amt. (g/70 kg daily)	Incidence of change*	
			Iproniazid† 50 mg/70 kg daily (9 patients)	Iproniazid placebo† (3 patients)
1-3		Glycine, 25	0/9	0/3
4-6	A	Phenylalanine, 20, and methionine, 20	3/9	0/3
		Glutamine, 40, and histidine, 20	0/9	0/3
		Tryptophan, 15	7/9	0/3
9		Glycine, 25	0/9	0/3
10-12	B	Methionine, 20	3/9	0/3
		Phenylalanine, 20	0/9	0/3
		Tyrosine, 20	0/9	0/3
19	C	dl-Methionine‡, 15, or placebo	0/5	0/0
25			Iproniazid increased 150 mg/70 kg daily	
29-30	D	Tryptophan, 7 or 15§	7/9	0/3
		Phenylalanine, 20	0/9	0/3
31-32	E	Methionine, 20‡, or 5-HTP or DOPA	3/3	0/1
			0/3	0/1
37	F	NH ₄ Cl, 15¶, or placebo§	0/4	0/0
			0/0	0/3

* Number of patients showing behavioral change/number of patients given loading substance. † Started at beginning of 2nd week and maintained throughout study. ‡ Given in gelatin capsules in four divided daily doses for 1 week. § In the 1st week six patients received the lower dose; in the 2nd week the remaining six patients received the higher dose. || 5-HTP (dl-5-hydroxytryptophan) and DOPA (l-dihydroxyphenylalanine) were given intravenously once daily starting with 6 mg and increasing in daily steps of 6 mg to 60 mg, then in steps of 12 mg, to 108 mg on the final day, except for two patients in whom injections were discontinued after 96 mg of DOPA and 24 mg of 5-HTP because of side effects. ¶ Enteric tablets in four divided daily doses for 1 week.

tric distress which usually accompanied methionine administration, since changes occurred when the amino acid was subsequently administered in capsules, which prevented the distress, and did not occur with administration of ammonium chloride, which produced both gastric distress and acidosis of greater severity.

The extent to which these clinical changes represent a biochemically induced acute flare-up of a chronic schizophrenic process on the one hand, or a toxic delirium superimposed upon chronic schizophrenia on the other, is, as yet, uncertain and is being further investigated (5).

Equivocal changes were noted in a few patients during the first tryptophan load in association with the lower dosage of iproniazid. During the higher dose of iproniazid, tryptophan administration was accompanied by mild to marked changes characterized primarily by mood elevation, increased involvement and extroversion, an early and transitory phase of somnolence, and more active deep tendon reflexes (6).

One guarded paranoid patient became euphoric and freely expressed delusions and amorous feelings. A withdrawn, almost mute patient became

verbal, intelligible, freely responsive to questions, and talked freely, though psychotically, about his activities and feelings. Five of the remaining seven patients showed similar but less marked changes, which were most evident in some by a sudden increase of hostility and depression when tryptophan was stopped. There was no overlap between marked methionine and tryptophan reactors.

Some of the patients, while receiving the higher dose of iproniazid, were given 5-hydroxytryptophan or l-dihydroxyphenylalanine in gradually increasing daily intravenous dosage up to 108 mg. Nausea, abdominal discomfort, or vomiting occurred in association with the higher doses of 5-hydroxytryptophan, and transitory hypertension, brachycardia, and ventricular extrasystoles with l-dihydroxyphenylalanine. Neither of these substances nor the other amino acids administered altered behavior in a manner detectable clinically.

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7 July 1960

Serum Glutamic Oxalacetic Transaminase Content in Hypothermia

Abstract. [When the body temperature of pentobarbitalized dogs was lowered, by surface-immersion technique, to 27°–26°C, elevations in serum glutamic oxalacetic transaminase were found only after a period of prolonged hypothermia (12 hours). When the animals were rewarmed, serum levels returned to normal. Histologic study of organs rich in glutamic oxalacetic transaminase revealed no necrosis. The cause for the elevations is not known, although increased membrane permeability secondary to prolonged cold may be a factor.]

Injury to tissue rich in glutamic oxalacetic transaminase results in elevated levels of this enzyme in serum (1). It has been felt that actual necrosis must occur for liberation of the enzyme from the cell into the serum. Recent evidence, however, suggests that, while ischemia is an important factor, necrosis per se is not required (2). It has been demonstrated that during hypothermia oxygen availability, transport, and use are adequate and no tissue damage develops (3). Physiological function essentially returns to normal. Histological studies of hypothermic animals have revealed necrosis, reportedly due to hypoxia (4). Another investigation differed, for no cellular damage was found (5).

The heart and the liver are particularly rich in glutamic oxalacetic transaminase cellular enzyme (6). Myocardial function is considered adequate during short-term hypothermia, with some question of adequacy after 6 hours of cold (7). Upon resuscitation of the animal, the hypothermic liver regains

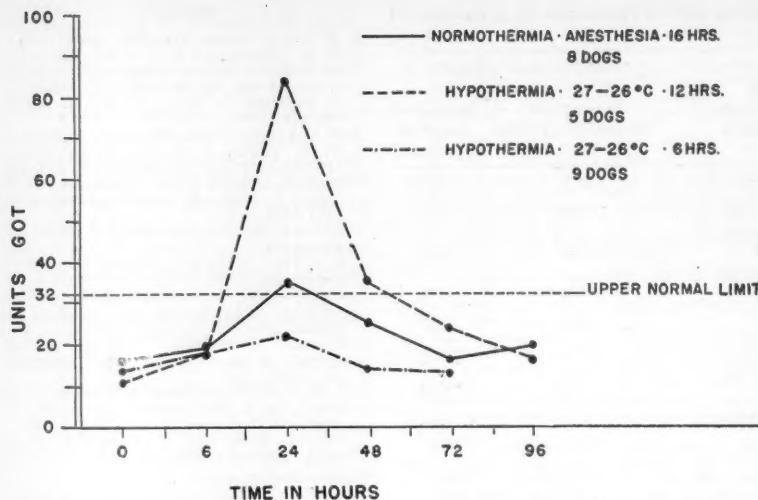


Fig. 1. Levels of glutamic oxalacetic transaminase (GOT) in prolonged hypothermia. Peak elevations occurred within 24 hours after arousal, with return to baseline levels within 72 hours. There was a tendency for serum glutamic oxalacetic transaminase to rise in the normothermia control animals.

its activity except for a lag in cellular oxidation (8).

Since a high level glutamic oxalacetic transaminase in the serum suggests tissue derangement, possibly due to necrosis, the present study was undertaken to determine serum levels of the enzyme in the pentobarbitalized dog cooled to 27°-26°C by the surface-immersion technique. Normothermic anesthesia controls were run for times approximately as long as those of the experiments involving prolonged hypothermia. Some of the animals breathed spontaneously, while others were given intermittent positive-pressure breathing with tank oxygen. Hypothermia was maintained for 6 and 12 hours in two separate groups, after which the dogs were rewarmed. Samples of arterial or venous blood, or both (the level of glutamic oxalacetic transaminase was the same in both), were drawn before the animals were cooled and 6, 24, 48, 72, and 96 hours after they were rewarmed. Sections of the following organs were taken after the last blood sample: ventricle, liver, kidney, adrenal gland, and skeletal muscle. The amount of the enzyme was determined by the method of Karmen (9). The upper limit of normal for this laboratory was established at 32 units.

All of the animals survived but were sacrificed for the tissue examinations. Spontaneously breathing and respiratory-supported animals demonstrated similar trends. Averages of the data are shown in Fig. 1. The control dogs showed tendency toward a rise in serum glutamic oxalacetic transaminase

with the peak at 24 hours after the periods of anesthesia (8 and 16 hours) and return to normal in another 24 hours. In the treated animals there were no elevations until after 12 hours of cooling. Within 72 hours serum levels of the enzyme were normal, with one exception, which returned to normal in the next 24 hours. Histological examination revealed no necrosis in the tissue studied and no unusual morphological differences between the control and the treated groups.

The reason for the elevations in serum glutamic oxalacetic transaminase, both in the anesthetized and the hypothermic groups, is not clear, especially in the absence of cellular necrosis. Tissue oxygenation does not appear to be a factor, since no differences were found between the respiratory supported and the unsupported animals. The intracellular content of the enzyme is high, and a large gradient exists between the cell and the serum. It may be speculated that membrane permeability to the enzyme was altered sufficiently to permit escape of the enzyme into the serum, under the conditions of this study. Increased membrane permeability with increased flux of ions during hypothermia has been demonstrated (10). Cold may affect the rate of transamination.

The effect observed in this study, whatever its cause, is transient, and apparently the normal gradient is reestablished within 72 hours after recovery from hypothermia. Evaluation of physiological function during hypothermia has been limited to short-term obser-

vations, and, generally, function (certainly oxygenation) appears to be adequate. Effects from prolonged exposure to cold may prove serious. If elevation of serum glutamic oxalacetic transaminase may be used as a yardstick of functional integrity, then one might predict that prolonged hypothermia might result in physiological disturbances.

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2 August 1960

Radiotelemetry of Physiological Responses in the Laboratory Animal

The measurement of physiological reactions in laboratory animals has long presented problems. Recording is usually accomplished under conditions varying from complete restraint to the partial restraint of extended wires. The animal may never grow entirely accustomed to the attachments, and this factor may constitute a source of stress. Restraint is reported in the literature as being extensively used as a stressor, the work of Selye on this topic being widely known. It is apparent, therefore, that measurement of physiological variables under conditions of even minimal restraint may yield a distorted picture of such responses. In addition, the cues inherently present in attachments of any kind, as well as the transfer of an animal to the study environment, are major—if not, at times, fatal—methodologic obstacles to classical conditioning. The development of the transistor offers new possibilities for classical conditioning.

With this in mind we set out to develop a system of radio telemetry for use as an adjunct to studies in classical conditioning. This methodological ad-

vance will permit the monitoring and recording of selected physiological reactions in intact, unanesthetized laboratory animals during their normal daily routines in a simulated normal environment uncontaminated by the intervention of the experimenter and experimental procedures, except for planned changes in the controlled environmental chambers. Transistorized, miniaturized, battery-powered packages are being developed in our laboratories and permanently implanted in laboratory animals. The modulation of the radio-frequency carrier with biological information permits short-distance propagation of selected physiological activities, with the possibility also of providing remote control of selected stimuli (1). Continuous measurement of physical conditions within the environmental chambers—ambient temperature, humidity, air ionization, barometric pressure, air velocity, light intensity, chemical composition of the air within the chambers, and such other physical parameters as may be shown to be significant—may be recorded along with the physiological activity specific to the animals. The behavior of the animals may be observed by a remote visual system. Classical conditioning experiments with animals may be conducted over periods of weeks and months, with observation periods before, during, and after experimental manipulation. We feel this innovation, growing out of the technique of telemetering, to be important and an improvement over techniques more familiar to those engaged in classical conditioning investigation.

Figure 1 (top) shows a signal output of respiration from a laboratory rat, obtained by means of the accelerometer principle incorporated into a small capsule (Fig. 1, bottom left) very much like that reported by Mackay (2). A brass pellet mounted on a rubber diaphragm near the oscillator coil modulates the radio-frequency carrier. We have found that we can obtain a radio-frequency signal of 6.8 Mcy/sec and of about 250 μ v at the antenna terminals of the receiver, with the circuit shown in Fig. 1 and battery current of 200 μ a. This by no means represents a lower limit to power requirements, but rather an arbitrary stopping point for the moment. The characteristics of the transistor are such that an increase or decrease in power is effected by changing the collector voltage and adjusting the emitter bias

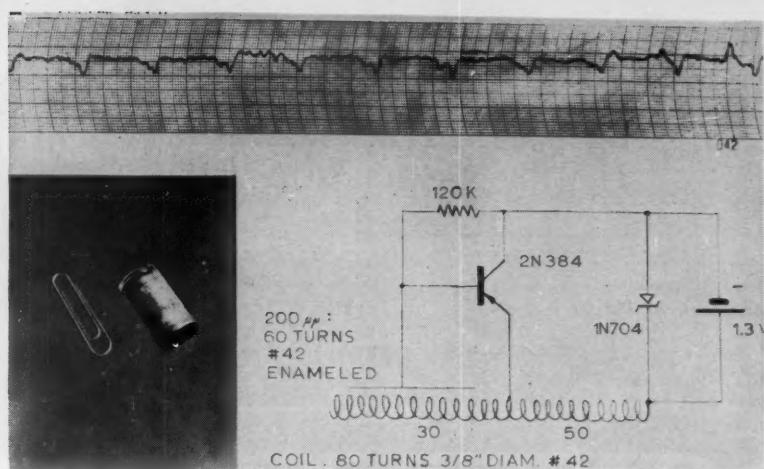


Fig. 1. (Top) Pen recording showing respiration and heartbeat on a base line of body temperature. (Bottom, left) Photograph of the active telemetering capsule. (Bottom, right) circuit diagram of the transistor transmitter.

to increase or decrease the collector current. There is no reason that the current cannot be of the order of 50 μ a, if this is permitted by the radio-frequency noise level within the environmental chamber. A transistorized, miniature, radio-frequency amplifier can be constructed within the antenna probe and supplied with low-voltage d-c power through the coaxial cable. We have found that, by exciting the capsule from an external source of radio-frequency power of about 3.9 Mcy/sec from a 100-watt exciter, about 2 ma of reverse current can be realized in the battery circuit within the capsule, in which we have included the Zener diode rectifier shown in the circuit diagram (Fig. 1). A rechargeable dry cell is being tested for the planned indefinite implantation of the capsule.

After the transmitter has been assembled and dipped in toughened paraffin, it is inserted by surgical procedures into the abdominal cavity of the rat, where ballistic movements are sensed by the accelerometer. The signal is picked up by a "ferri-loop-stick" antenna mounted within the environmental chamber and conducted through a coaxial cable to a communications receiver. The incoming frequency-modulated signal is mixed with a beat-frequency oscillator within the receiver, or with another radio-frequency signal from a frequency meter. The audio output of the receiving system is recorded

on magnetic tape for subsequent playback and data analysis.

While the base line of the trace (Fig. 1, top) is an accurate (0.2° per 1000 cy/sec of audio output) and reproducible (± 0.5 percent) measure of the core temperature of the animal, a transistor alone will not detect rapid changes of temperature. The response time constant of the radiosonde used at this writing is of the order of 100 seconds. It is therefore necessary to introduce a thermistor transducer into the circuit to effect this measurement.

The improvement in techniques inherent in microminiaturization and telemetry permit the coupling of classical conditioning experiments into an on-line digital computer to form a closed-loop systems approach to experimentation. This, in conjunction with automatic data processing and reduction, would seem to lead to qualitatively different testing of old and new hypotheses with multiple independent and dependent variables, under conditions of experimental control previously impossible.

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26 September 1960

Association Affairs

Academy of Psychoanalysis

The Academy of Psychoanalysis, a new affiliate of the AAAS, was organized in 1956 to constitute a forum for inquiry into the phenomena of individual motivation and social behavior, to support research in psychoanalysis, and to further the integration of psychoanalysis in undergraduate and graduate medical education. Academy fellows are physicians who are graduates and faculty members of psychoanalytic schools and institutes. Scientific associates of the academy are physicians and doctors of philosophy of allied disciplines whose major interests are teaching and research. The academy does not set standards for psychoanalytic training. Among the present total of 342 fellows and 55 scientific associates are members from Canada, South America, Mexico, and Europe.

The proceedings of the semiannual academy meetings, edited by Jules H. Masserman, are published annually in book form. The proceedings volumes published to date, under the series title *Science and Psychoanalysis*, are as follows: vol. 1, *Integrative Studies* (1958); vol. 2, *Individual and Family Dynamics* (1959); vol. 3, *Psychoanalysis and Human Values* (1960) (Grune and Stratton, New York).

Academy meetings follow a pattern. On the first day, papers on a selected theme are presented and discussed. On the second day, papers not necessarily relevant to that theme are presented and discussed in like manner.

The winter meeting this year will be held at the Hotel Biltmore, New York, 10 and 11 December. The theme of the meeting is "The Role of Values in the Psychoanalytic Process." Papers will be presented as follows: "Values, truth, and psychoanalysis," John R. Reid (Baltimore); "Values, maturation, and health," A. H. Maslow (Waltham, Mass.); "Value differences between patient and psychoanalyst," Janet MacKenzie Riach (New York); and "Values, identity, and the psychoanalytic process," Marianne H. Eckardt (Washington, D.C.).

The various schools of psychoanalytic thought are represented by the

elected officers of the academy for the year 1960-61, who are all physicians. They are as follows: president, Frances S. Arkin (New York); president-elect, Roy R. Grinker (Chicago); past-president, John A. P. Millet (New York); secretary, Joseph H. Merin (New York); treasurer, John L. Schimel (New York); trustees, Nathan W. Ackerman (New York), Ralph M. Crowley (New York), Don D. Jackson (Palo Alto, Calif.), Alexander Reid Martin (New York), May E. Romm (Beverly Hills, Calif.), Leon Salzman (Washington, D.C.), Leon J. Saul (Media, Pa.), and Natalie Shainess (New York). Franz Alexander (Los Angeles) is chairman of the recently appointed committee on education and research.

Montague Ullman is the academy's representative on the AAAS Council. Ullman received his medical education at New York University and his training in psychiatry and psychoanalysis at the New York State Psychiatric Institute and the New York Medical College. He is a diplomate of the American Board of Psychiatry and Neurology and is on the faculty of the New York Medical College. Ullman is a Fellow of the AAAS and has been interested in the research and educational application of psychoanalytic principles.

JOSEPH H. MERIN

Academy of Psychoanalysis,
New York, New York

American Speech and Hearing Association

The American Speech and Hearing Association, a new affiliate of the AAAS, is the national scientific and professional association in the field of speech pathology and audiology to which most of the qualified specialists in this field belong. Its basic purposes are to advance speech and hearing as an area of science and to promote human welfare through study of the processes and disorders of individual human communication. The association accomplishes these aims by: providing for the continuing improvement of standards

for the education and professional training of research and clinical personnel in speech pathology and audiology and in basic research in speech and hearing; the encouragement of research and the continuous review and refinement of theory concerned with the functions and disorders of communication; activities designed to improve the competence of speech and hearing specialists; and the fostering of high standards of professional ethics and conduct for its members.

The affairs of the association are administered by the executive secretary and his staff in the national offices, which are located at 1001 Connecticut Avenue, NW, Washington 6, D.C.

The association has a current membership of approximately 6200. It began as the American Academy of Speech Correction in 1925 and assumed its present name in 1947. Annual conventions have been held since 1925. The variety and vitality of these programs have contributed significantly to the growth of the profession which the association serves.

The association publishes the *Journal of Speech and Hearing Disorders*, the *Journal of Speech and Hearing Research*, experimental and clinical *Monographs*, and *Asha*. It is the association's editorial policy to provide a balanced program of publication of research, theory, and clinical principles and methodology for scholars, students, research workers, and clinicians concerned with speech and hearing. Recent *Monograph Supplements* include: *Effects of Noise on Man*, by Karl Kryter; *The Disorder of Articulation: A Systematic Clinical and Experimental Approach*, by Robert Milisen and associates; *The Problem of Stuttering in Certain North American Indian Societies*, by Joseph Stewart; and *Research Needs in Speech Pathology and Audiology*, by the committee on research of the American Speech and Hearing Association. The association also publishes a *Directory of Members*.

In collaboration with Gallaudet College, the association has recently taken steps to effect the quarterly publication of *Deafness Speech and Hearing Abstracts*, covering relevant current professional literature, together with a one-volume bibliographic index of all material published in the fields of deafness, speech, and hearing prior to January 1960.

The committee on research of the association is presently directing two major nationwide studies, one a national survey and appraisal of public-school speech and hearing services and the other an investigation of the professional preparation of speech pathologists and audiologists in the United States.

HYPERFINE STRUCTURE IN ORGANIC FREE RADICALS

BY EPR

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Interaction in organic free radicals of the unpaired electron with the magnetic moments of the protons frequently gives rise to well defined hyperfine structure. Often this structure permits identification of an unknown radical. One may also extract detailed information on electron wave functions from this observed hyperfine splitting.

EXAMPLE

Electrolytic production of negative ions.

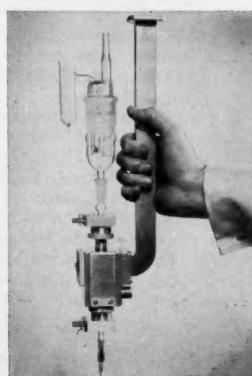
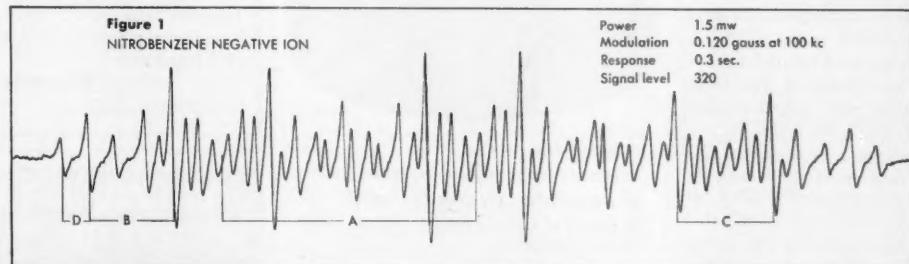


Figure 2 Electrochemical cell as used with the spectrometer.

Recently Maki and Geske¹ reported a radically new and important application of EPR. They showed that it was now possible to observe directly the one electron transfer process in the electrolytic reduction of nitrobenzene to the negative ion. They prepared the negative ion by constant potential electrolysis of nitrobenzene in a solution of acetonitrile with tetra-n-propylammonium perchlorate as supporting electrolyte. Such methods of production of negative ions are preferable to the metal reduced systems in that the EPR spectrum can be interpreted completely without complication of interaction by the metal.

Figure 1 shows the spectrum of the nitrobenzene negative ion when generated from a solution of benzonitrile and tetra-n-propylammonium perchlorate². Forming the ion in benzonitrile seems to improve the resolution of the spectrum obtained. The predicted 54 lines are easily observed.

Splitting (A) represents the nitrogen coupling constant which is 10.3 gauss. Splitting (B), (C) and (D) correspond to the coupling constants of the ortho, para and meta hydrogens. The electrochemical cell used in the generation of the negative ions is illustrated in Figure 2 and is a modification of the V-4548 aqueous sample cell accessory.

¹ JACS 82, 2671 (1960).

² Sample donated by Dr. R. Adams, University of Kansas.

For literature which fully explains the 100 kc EPR Spectrometer and its application to basic and applied research in physics, chemistry, biology and medicine, write the Instrument Division.



VARIAN associates

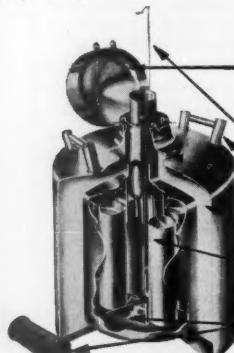
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The 1960 officers and officers-elect of the American Speech and Hearing Association are as follows: President, Stanley Ainsworth, professor of speech correction and chairman of the program on exceptional children at the University of Georgia; president-elect, Paul Moore, director of the Gould Research Laboratories, William and Harriet Gould Foundation of Chicago, and professor of speech pathology at Northwestern University; executive vice-president, Jack Matthews, chairman of the speech department and director of the speech clinic at the University of Pittsburgh; chairman of the publications board, Wendell Johnson, professor of speech pathology and psychology at the University of Iowa; vice-president, Jack Bangs, director of the Houston Speech and Hearing Center; vice-president-elect, Duane Sprickerbach, professor of speech pathology and audiology at the University of Iowa; and executive secretary, Kenneth O. Johnson, national office of the American Speech and Hearing Association in Washington, D.C. The newly elected AAAS Council representative is Mildred C. Templin, professor, Institute of Child Development and Welfare, University of Minnesota.

KENNETH O. JOHNSON
Washington, D.C.

Forthcoming Events

January

24-27. Society of Plastics Engineers, 17th annual conf., Washington, D.C. (T. A. Bissell, SPE, 65 Prospect St., Stamford, Conn.)

25-27. Mathematical Assoc. of America, annual, Washington, D.C. (H. L. Alder, Dept. of Mathematics, Univ. of California, Davis)

26-27. Western Spectroscopy Conf., 8th annual, Pacific Grove, Calif. (R. C. Hawes, Applied Physics Corp., 2724 S. Peck Rd., Monrovia, Calif.)

27-28. Royal College of Physicians and Surgeons, annual, Ottawa, Ontario, Canada. (T. J. Giles, 150 Metcalfe St., Ottawa)

28-30. Control of the Mind, symp., San Francisco, Calif. (Dept. of Continuing Education in Medicine, Univ. of California Medical Center, San Francisco 22)

28-31. Infertility, sectional meeting, Intern. Fertility Assoc., Acapulco, Mexico. (M. L. Brodny, 4646 Marine Dr., Chicago 40, Ill.)

29-3. American Inst. of Electrical Engineers, winter meeting, New York, N.Y. (E. C. Day, AIEE, Technical Operations Dept., 33 W. 39 St., New York 18)

30-3. Clinical Cong. of Abdominal Surgeons, Miami Beach, Fla. (B. F. Alfano, 663 Main St., Melrose 76, Mass.)

30-4. American Library Assoc., mid-winter meeting, (Mrs. F. L. Spain, New York Public Library, 20 W. 53 St., New York, N.Y.)

31-4. American Assoc. of Physic Teachers, New York, N.Y. (F. Verbrugge, 135

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Main Engineering, Univ. of Minnesota, Minneapolis)

31-4. American Physical Soc., annual, New York, N.Y. (K. Darrow, APS, Columbia Univ., 116th St. and Broadway, New York)

February

1-3. Solid Propellant Rocket Conf., American Rocket Soc., Salt Lake City, Utah. (R. D. Geckler, Aerojet-General Corp., P.O. Box 1947, Sacramento, Calif.)

1-3. Winter Military Electronics Conv., 2nd, Inst. of Radio Engineers, Los Angeles, Calif. (A. N. Curtiss, IRE Business Office, 1435 S. La Cienega Blvd., Los Angeles 35)

1-4. American Physical Soc., annual, New York, N.Y. (K. K. Darrow, APS, 538 W. 120 St., New York 27)

2-4. Congress on Administration, 4th annual, Chicago, Ill. (R. E. Brown, American College of Hospital Administrators, 840 N. Lake Shore Dr., Chicago 11)

6-8. American Acad. of Allergy, 17th annual, Washington, D.C. (J. O. Kelly, 756 N. Milwaukee St., Milwaukee 2, Wis.)

6-8. Geodesy in the Space Age, symp., Ohio State Univ., Columbus. (W. A. Heiskanen, Ohio State Univ., 1314 Kinnear Road, Columbus 12)

6-10. British Medical Assoc., annual, Auckland, New Zealand (E. Grey-Turner, B.M.A., Tavistock Sq., London, W.C.1)

9-15. Second Allergy Conf., Nassau, Bahamas. (I. M. Wechsler, P.O. Box 1454, Nassau)

13-16. American Soc. of Heating, Refrigerating and Air-Conditioning Engineers, Chicago, Ill. (R. C. Cross, 234 Fifth Ave., New York 1)

14-15. Conference on Microdosimetry, 2nd, Rochester, N.Y. (N. Kreidl, Bausch & Lomb Optical Co., Inc., Rochester 2)

15-17. International Solid-State Circuits Conf., Philadelphia, Pa. (J. J. Suran, Bldg. 3, Room 115, General Electric Co., Electronics Park, Syracuse, N.Y.)

16-18. Biophysical Soc., annual, St. Louis, Mo. (W. Sleator, Dept. of Physiology, Washington Univ., St. Louis 10)

22-25. American Educational Research Assoc., annual, Chicago, Ill. (G. T. Buswell, 1201 16th St., NW, Washington 6)

23-25. American Orthopsychiatric Assoc., annual, New York, N.Y. (Miss M. F. Langer, 1790 Broadway, New York 19)

23-25. Fifteenth Annual Symp. on Fundamental Cancer Research, Houston, Tex. (Publications Dept., Univ. of Texas M.D. Anderson Hospital and Tumor Inst., Texas Medical Center, Houston 25)

23-25. Symposium on Molecular Basis of Neoplasia, Houston, Tex. (Publications Dept., Texas Medical Center, Houston 25)

26-1. American Inst. of Chemical Engineers, natl., New Orleans, La. (F. J. Van Antwerp, AICHE, 25 W. 45 St., New York 36)

26-2. American Inst. of Mining, Metallurgical, and Petroleum Engineers, annual, St. Louis, Mo. (AIME, 29 W. 39 St., New York 18)

27-3. Conference on Analytical Chemistry and Applied Spectroscopy, 12th, Pittsburgh, Pa. (L. P. Melnick, U.S. Steel Corp., Monroeville, Pa.)

(See 16 December issue for comprehensive list)

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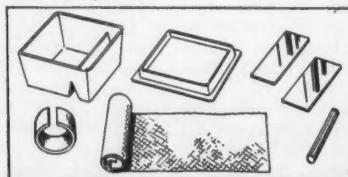
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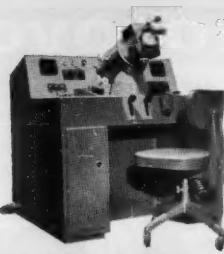
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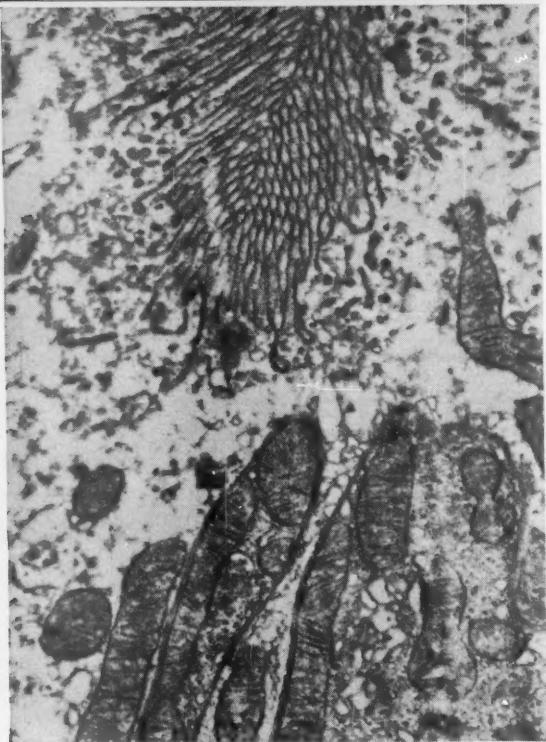
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appendices. The decay-rate calculator is a circular-disk type that provides information on remaining activity when half-life and original activity are inserted. The handbook and the calculator are both available free of charge in small quantities. (Atomic Energy of Canada Ltd., Dept. Sci12, Commercial Products Div., P.O. Box 93, Ottawa, Can.)

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JOSHUA STERN

National Bureau of Standards,
Washington, D.C.

Letters

(Continued from page 68)

way, due to the Faraday effect of the building. If the authors found that there was a field within the building, there would still be a Faraday effect applicable to the culture medium within the bottles.

In experiments to study progeny yields one should also consider the factors which affect behavior and oviposition of the parents, in addition to those factors which might act on larval development. In this regard, one of

the "unknown factors" Levengood and Shinkle associated with barometric pressure changes might be air ions. Air ion densities are known to change with different kinds of weather, and I have found that positive air ions can influence the activity of adult blowflies.

DONALD K. EDWARDS
Forest Biology Laboratory, Canada
Department of Agriculture, Victoria

We would first like to comment on Edward's question concerning variability in progeny yields from cultures subjected to the influence of the electric field. The 16 cultures in the elec-

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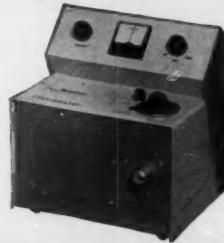
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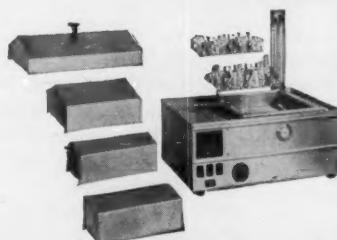
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tric field disclosed a mean deviation of 32.3 percent, whereas the control-group data showed a mean deviation of 52.4 percent. Also, the average progeny yield of flies grown in the electric field was 35.4 percent higher than the average for the control groups. When we made the statement that the electric field appears to provide a certain amount of protection we were, of course, referring to this decrease in variation and increase in progeny yield from cultures within the electric field. These figures were not included initially, due to the inevitable discrepancy between the al-

lotted space and the amount of information one wants to provide.

We feel that the outer envelope of glass (the culture bottle) is the most important factor in reducing the field strength. The dielectric constant of glass varies between 5 and 10, and in our calculations we used a value of 8 to be on the high side. The magnitude of the electrostatic field approximated from the physical dimensions is 7×10^{-9} coul; the exponent was inadvertently changed from -9 to 2 when the manuscript was initially compiled. If Edwards prefers that the field be expressed

in terms of electrostatic field intensity, then it is again necessary to make approximations because of shape factors. The general field intensity in which the bottles were placed was about 2.5×10^6 v/m. The directional field may be of sufficient strength to produce ionic drift toward the electrodes and decrease the density of air ions within the bottles. The effects of ions with known specific charges on physiological processes have been previously reported by Krueger *et al.* [Proc. Soc. Exptl. Biol. Med. 102, 355 (1959)].

Ionizing radiation produced by cosmic rays is known to affect the electric and geomagnetic fields surrounding the earth. These fluctuating electric-field effects at high altitudes are believed to influence the production of less energetic ions at the earth's surface. The ionization at lower altitudes is also affected by barometric pressure, and the increase in ionization with decreasing pressure is an absorption effect. It is conceivable that this increase in ionization with decreasing barometric pressure could account for the pronounced decrease in the progeny yields from control cultures. It is these air-ion effects which we feel are significant in causing the variations, and not a Faraday effect as suggested by Edwards.

In reviewing the literature, no reference could be found pertaining to the effects on *Drosophila melanogaster* of varying barometric pressure. Pictet (1904-21) makes no reference to *D. melanogaster* but mentions only emergence of adult insects from pupal tissues. He mentions that the majority of adults emerge on the fall of the barometer; this could be explained possibly by brittle pupal cases, the result of greater evaporation of liquids at the lower pressure. This has nothing to do with matings in *Drosophila*. Also, Parman has stated that adult insects seem to emerge during periods of high pressure [D. C. Parman, *J. Econ. Entomol.* 13, 339 (1920)]; thus, the effect of pressure on emergence appears to be in question.

As Edwards stated, Stephen and Bird observed an increase in oviposition in *Pieris rapae* at low pressures; however, an aspirator was used to produce the variations in pressure. This creation of artificial pressure variations would not correlate with changes such as air-ion variations occurring with natural fluctuations in atmospheric pressure. Also, these experimenters consider only one stage in the life cycle of the insect, whereas our experiments are based on complete life cycles of a number of generations of *D. melanogaster*.

W. C. LEVENGOOD
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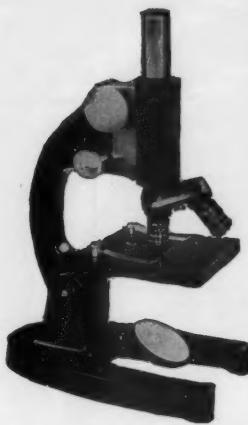
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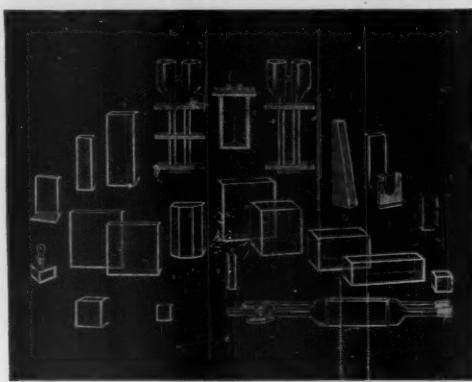
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The Coriolis Effect is in force in outer space, too. If a space vehicle is rotated in order to establish artificial gravity, the necessarily short radius of the rotation causes a Coriolis force. This creates orientation problems for a human occupant. To eliminate this difficulty, a scientist at Lockheed Missiles and Space Division conceived the idea of connecting the vehicle to an auxiliary fuel tank by a half-mile-long cable. Thus, if the whole system is then rotated at a reduced speed around its center of mass gravity, the longer radius greatly minimizes the Coriolis force. Right now—on the drawing boards at Lockheed—is an enormously advanced space vehicle system which utilizes this concept, in addition to many others.

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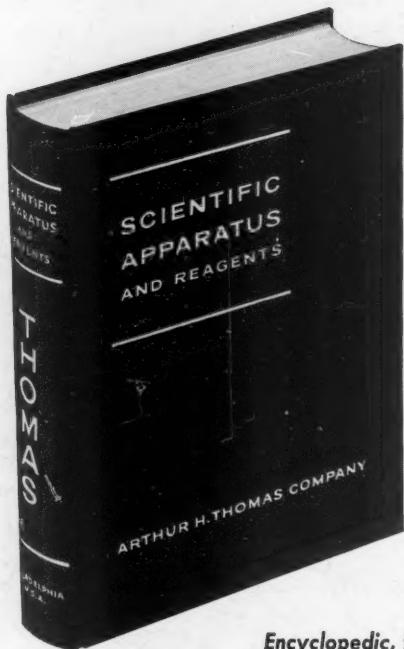
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